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To Dr. BATTY.

DEAR SIR,

IF the publication of the enclosed Case accords with the design of your work, you will oblige me by inserting it in your next Number. I am desirous that medical men should, as soon as possible, be put in possession of the facts relating to this Case, in order that they may judge of them for themselves, and act in future as that judgment may direct. I am, &c.

Bedford Row, Jan. 6. JOHN ABERNETHY.

——— Wrungel, a German, by trade a sugar-baker, of a sickly aspect and slender make, about 5 feet 7 inches high, and near 40 years of age, was admitted into St. Bartholomew's Hospital, on account of an aneurysm in the femoral artery, close to Poupart's ligament. This had arisen from a strain about three weeks before. The tumour at the time of admission was of the size of a small orange, and the blood contained in it was fluid; for it could be entirely expressed from the aneurysmal sac. At a consultation on the treatment of the case, I said that I did not think a surgeon warranted in tying the external iliac artery, till he was in some measure compelled to it by the progress of the disease, for the following reasons. 1st. An aneurysm, in proportion to its increase and duration, obstructs the passage of the blood through the natural and principal channels, and obliges it to circulate by other courses, which are enlarged according to the exigency of the case. It seems highly probable, that in proportion to the size of the artery which is tied, and the magnitude of the part to be nourished after that operation, so will be the degree of previous enlargement to these collateral channels, which is necessary to ensure its success. On this account the operation should be delayed longer in an inguinal aneurysm than in any other.

2dly, The operation of tying the external iliac artery must, in the present state of our knowledge, be considered as very serious in its nature, and uncertain in its event. I once tied this

veſſel when a man would otherwiſe have bled to death from the femoral artery; and though the limb was nouriſhed, the artery ulcerated. The operation was done a ſecond time in London, and the limb mortified: but no fair practical inference can, I am told, be drawn from the latter caſe, as the operation was poſtponed till mortification was as it were impending.

3dly. There is ſome chance in Aneuryſms of a cure ſpontaneouſly occurring from the cloſure of the artery above by the coagulation of the blood. To cite thoſe inſtances only which have come within my own knowledge, and which it ſeems right to mention, as it increaſes the ſtock of facts before the public; I have known ſuch a ſpontaneous cure take place twice in the popliteal artery, once in the arteria profunda femoris, and once in the axillary artery. For theſe reaſons it was agreed to poſtpone the operation in the caſe of the preſent patient till circumſtances ſhould appear to demand its performance.

Our poor patient therefore lay in the hoſpital during two months, in which time his diſeaſe gradually increaſed, and his health declined. Towards the latter part of the time he ſuffered a great deal of pain in the front of his thigh, which deprived him of reſt, and the whole limb was largely œdematous. Theſe ſymptoms would naturally ariſe from the preſſure which the aneuryſm muſt make on the anterior nerves and abſorbents of the thigh. The tumour had advanced towards the ſurface, and the ſkin had become ſlightly inflamed, yet the protruding part of the tumour was not of greater extent than when he was firſt admitted into the hoſpital, and no judgment could be formed of that part which was more deeply ſituated, on account of the general ſwelling of the thigh. The blood could even now be expreſſed from the prominent part of the tumour, and I felt anxious, leſt the obſtruction to the circulation in the main artrey ſhould not have been ſufficient to have obliged the blood to circulate by other channels. It deſerves to be remarked, that the aneuryſm may extend conſiderably beneath the faſcia of the thigh, cauſing pain and œdema by its preſſure, and yet that part which advances towards the ſurface may be of no great magnitude.

The patient's ſufferings increaſed conſiderably during the week preceding the operation, ſo that he declared his preſent ſtate was almoſt inſupportable, and ſolicited that ſomething might be done to change it either for the better or the worſe. He never, however, was able to explain the cauſe of this uncommon degree of anxiety and inquietude.

The operation was undertaken on Saturday the 24th of October. An inciſion of three inches in length was made through the

the integuments of the abdomen, beginning a little above Poupart's ligament, and being continued upwards; it was more than half an inch on the outſide of the upper part of the abdominal ring, to avoid the epigaſtric artery. The aponeuroſis of the external oblique muſcle being thus expoſed, was next divided in the direction of the external wound. The lower part of the internal oblique muſcle was thus uncovered, and the finger being introduced below the inferior margin of it and of the tranſverſalis muſcle, they were divided by the crooked biſtoury for about one inch and a half. I now introduced my finger beneath the bag of the peritonæum, and carried it upwards by the ſide of the pſoas muſcle, ſo as to touch the artery about two inches above Poupart's ligament. I took care to diſturb the peritonæum as little as poſſible, detaching it to no greater extent than would ſerve to admit my two fingers to touch the veſſel. The pulſations of the artery made it clearly diſtinguiſhable from the contiguous parts, but I could not get my finger round it with the facility which I expected. This was the only circumſtance which cauſed any delay in the performance of the operation. After ineffectual trials to paſs my finger beneath the artery, I was obliged to make a ſlight inciſion on either ſide of it, in the ſame manner as is neceſſary when it is taken up in the thigh, where the faſcia which binds it down in its ſituation is ſtrong. After this I found no difficulty in paſſing my forefinger beneath the artery, which I drew gently down, ſo as to ſee it behind the bag of the peritonæum. By means of an eyed probe two ligatures were conveyed round the veſſel; one of theſe was carried upwards as far as the artery had been detached, and the other downwards: they were firmly tied, and the veſſel was divided in the interſpace between them. Nothing further remained than to cloſe the external wound, which was done by one ſuture, and ſome ſtrips of ſticking plaiſter. The threads of the upper ligature were left out of the wound above the ſuture which cloſed its edges, and thoſe of the lower beneath.

A few remarks on this operation may be permitted. To divide the parietes of the abdomen, puſh aſide the peritonæum, and tie the external iliac artery by the ſide of the pſoas muſcle, is an operation more formidable in ſound, and on its firſt propoſition, than it is in reality. It is performed almoſt without ſhedding blood, ſo that the principal circumſtances of it are very evident. When I formerly performed this operation, I was urged to it by immediate neceſſity: I tied the artery much higher than in the preſent caſe, diſturbed the peritonæum in a greater degree, and, contrary to my own principles, I did not divide the artery. In the preſent caſe, having time to deliberate upon the

ſteps of the operation, I detached merely ſo much of the peritonæum, as enabled me to reach the artery as far as I conveniently could above Poupart's ligament; but not ſo far as to make it difficult to aſcertain that I ſurrounded the artery only with my finger, without injuring any of the adjacent parts, nor ſo far but that I could draw down and diſtinguiſh the artery which I included in the ligature. The remembrance of the ſwelling in the external iliac glands, and of the ulceration of the artery in the former caſe, led to this difference of conduct.

The poor man was greatly exhauſted by the operation, and his leg, which had been chilled by expoſure during the operation, continued very cold for a long time afterwards. It was wrapped up in flannels, to prevent the diſſipation of its own heat; but I would not apply any artificial warmth to reſtore its temperature, leſt it ſhould act as a ſtimulant.

He could not compoſe himſelf after the operation, nor did he ſleep during the night, ſo that on the following day his ſtate was very unpromiſing. His pulſe beat 160 in a minute, his tongue was covered by a dark brown fur; he looked agitated, and a purging took place, which was not reſtrained till the following night by a cordial and opiate mixture. Reſpecting his pulſe, it is proper to mention that it beat 120 moſt days in the week preceding the operation.

His thigh was as warm as that of the ſound ſide, his leg cooler than the oppoſite one, and his foot many degrees colder. He had however perfect ſenſation in his toes, and power of moving them. The leg and foot were rubbed with oil three or four times a day, in order to prevent any ſtagnation in the veins, and to diminiſh perſpiration. It was well covered as before by flannels.

On Monday, the 2d day, (Oct. 26) the pulſe was leſs frequent: he had ſlept a good deal during the night, and ſeemed ſtupified by opium; but on the whole ſo little better, that I concluded he would gradually ſink in conſequence of the ſhock of the operation. The temperature of the limb was a little increaſed. The man however took bread and milk and other food in moderate quantities, whenever it was offered to him: the purging having ceaſed, the quantity of the opiate was diminiſhed. He rather improved in the evening, and reſted well during the night; ſo that on (October 27) the third day after that of the operation every circumſtance wore a favourable aſpect. His pulſe did not exceed 100, and was moderately firm and full; his appetite had increaſed: the temperature of the limb was a good deal augmented, ſo that his foot was ſcarcely colder than that of the ſound ſide; and the œdema of the limb was conſiderably diminiſhed. I now dreſſed his wound, in which he

had

had not complained of pain, nor of any tendernefs, when the furrounding parts were compreffed. The incifion appeared but as a line, except at the neighbourhood of the ligatures, where it was a little open, and from whence there iffued a moderate quantity of as healthy pus as I had ever feen. The furrounding parts were perfectly natural both in appearance and fenfation. On the 4th day (October 28) he was ftill better: his pulfe 90; his appetite good; his fleep found; and his limb leffening in fize, and increafing in warmth. The ftudents at the hofpital had dreffed the wound before my arrival, and reported that the difcharge was tinged with blood.

On the 5th day (October 29) he was ftill better, his pulfe being but 80 when I counted it. The wound and contiguous parts looked remarkably well, but a bloody fanies was difcharged, which I felt unable to account for.

On the 6th day (October 30) the ftate of his health and limb continued as well, if not improving. The bloody difcharge however had increafed in quantity, infomuch that it ran through the coverings of the wound, and foiled the bed; it had alfo become fœtid. From the firft occurrence of this bloody difcharge I felt confiderable uneafinefs refpecting it. I could not believe that an healthy wound would fecrete fuch a fanies, and I felt apprehenfive left the wound fhould fpread from difeafe. Nothing however took place to confirm this idea. It feemed probable alfo that if the aneuryfmal fac were not entire, fome of the blood being expofed to the air might tinge the difcharge from the wound, and grow putrid. I frequently preffed on the tumour, but could prefs no blood from the wound. In this ftate of uncertainty it was, however, pleafing to obferve, that the patient's health continued in every refpect better than could reafonably have been expected.

The circumftances of the cafe remained very much the fame during the 7th and 8th days after the operation. On the morning of the 9th, (Nov. 2,) when I came to the hofpital, I met Mr. Blicke, who told me that the poor German was dying; intelligence which equally furprized and fhocked me.

He was indeed in a dreadful ftate, appearing like a man far advanced in typhus fever. His pulfe was 150; his tongue covered with a brown fur; his intellect wavering, and the action of his mufcles tremulous. On examining the wound, with a view to difcover the caufe of this great and fudden alteration, and preffing on the tumour beneath Poupart's ligament, I forced out a great quantity of blood, rendered fluid and highly fœtid by putrefaction, infomuch that it inftantly blackened the probe with which it accidentally came in contact.

The caufe and circumftances of the bloody difcharge were now

now made clear; the ſurface of the expoſed coagulated blood of the aneuryſm had at firſt tinted the diſcharge from the wound, then had, by gradual diſſolution, been more plentifully commixed with it, and given it a degree of putridity. Till however the whole maſs had become putrid, and had been converted in conſequence into fluid, it could not be forced out from beneath Poupart's ligament, when preſſure was made on the tumour; nor did it till that period excite inflammation in the ſurrounding parts by its acrimony, or derange the conſtitution by its abſorption.

After entirely expreſſing the putrid blood, I waſhed out the cyſt with warm water, till it returned untinged. The relief which was by theſe means afforded to the poor man, was very ſtriking and conſiderable. His pulſe became moderate, his intellect clear; he had ſome refreſhing ſleep, and again took food in moderate quantities. On the following day, when the integuments beneath Poupart's ligament were compreſſed, a conſiderable quantity of fœtid diſcharge and air were forced out. It was not however at all tinged with blood, and appeared to me to be merely the ſecretion from the cyſt, which had contained the blood. I directed that this diſcharge ſhould be preſſed out, the cavity ſyringed, and a poultice applied three times a day; but finding it ſtill ſecreted in conſiderable quantity, I thought it right to make an opening into the cyſt beneath Poupart's ligament, to afford it a more ready exit. No abatement in the quantity, or alteration in the quality, was however remarked; it ſeemed to be ſuch as a ſloughing ſore commonly furniſhes.

This fever came on (November 1) on the evening of the eighth day after that of the operation; and I am convinced it would have ſpeedily deſtroyed him, had not the cauſe been detected and removed. The powers of his conſtitution rallied again; his pulſe was firm, and often not more than 100; he took ſufficient food, and ſlept moderately well. But the part, as has been ſaid, did not go on well, and ſeemed to prevent any increaſe of ſtrength. For a week I was not without hopes that ſome favorable change might happen, but afterwards I loſt all expectations, as his already much reduced powers were ſtill further declining; nevertheleſs, he held out more than another week, when he died on November 16, the 23d day after the operation. A few days before his death both ligatures came away with the dreſſings.

Dissection.

A very ſlight adheſion had taken place between the ſigmoid flexure of the colon, and that part of the peritonæum which was

oppoſite

opposite to the wound, but there was no other appearance of that membrane, or of the bowels, having suffered any inflammation in consequence of the operation. The peritonæum was separated from the loins, and from the posterior half of the left side of the diaphragm, by a considerable collection of blood, which extended below to Poupart's ligament, and communicated under that ligament by a small aperture with the aneurysmal sac. This opening was situated in the direction of that crevice which is found between the internal iliac and psoas muscles. The only rational explanation that can be given of the formation of this collection is, that the blood had burst its way from the aneurysmal sac in the vacancy between the muscles just mentioned; after which it would readily and extensively separate the peritonæum in the manner described. I am inclined to attribute to this circumstance the undefinable disturbance of health, which the poor patient suffered during the week preceding the operation. It may, perhaps, excite surprize that this collection did not become putrid.

No particular account can be given of the aneurysmal sac beneath Poupart's ligament, since it, and the contiguous parts, had sloughed in consequence of the irritation of the putrid blood. A small aperture had been made by this sloughing in the front of the orbicular ligament of the hip joint, and a small extent of the thigh bone was, by the same cause, deprived of its periosteum.

A bougie was passed from the lower end of the femoral artery into the sac.

The extremities of the external iliac artery, which had been divided in the operation, were united together by a fine new-formed substance; the sides of each extremity were perfectly closed, and a small plug of coagulated blood was found in each.

Having thus given as brief an account as I am able of the circumstances of this case, as they appeared to me, I cannot conclude without mentioning the observations of others, particularly as they may assist in suggesting rules of conduct for future operations on similar cases. It has been said that the irritation of the aneurysmal bag was probably a spontaneous occurrence, and not the effect of the acrimony of the putrid blood. But the suddenness of this attack, the manifest existence of a cause sufficient to produce it, and the total absence of such an occurrence in all other cases of aneurysm, render this supposition highly improbable.

It has also been imagined that part of the discharged blood might have returned from the lower end of the artery. This latter opinion is very improbable, since after the complete removal of the blood, none returned by that channel: and in the former case

case which I mentioned, none returned by the inferior part of the artery; though the area of it was still of its natural dimensions, and unobstructed. This latter observation had tended to diminish my confidence in the powers of the communicating channels, and made me wish to defer the performance of the operation as long as possible. It seems evident that in the present instance it was too long delayed.

It would be desirable in future to perform the operation before an extensive diffusion of blood had taken place; indeed, could the adequateness of the collateral arteries for the supply of the limb be established, it would be proper to operate before the artery had burst.

It deserves to be considered, whether it might not be right at the time of the operation to open the aneurysmal bag, and remove the blood. I should, however, be inclined to postpone this; for, perhaps, no necessity might exist, no putrefaction might take place. A few days will determine the degree of life of the limb, and will make a wound less likely to ulcerate or slough. Should signs of the putrefaction of the blood ensue, or the probability of such an occurrence become evident, I should think it necessary to make a small opening into the aneurysmal bag for the removal of the contained blood. This being done, if no blood came from the lower orifice of the artery, there would be no necessity for tying it. Such are the observations that have occurred to me on this subject; I have laid both those and the facts before the public as early as possible, that medical men might judge of the circumstances of the case themselves, and form their opinions accordingly.

To the Editors of the Medical and Physical Journal.

Gentlemen,

I HAVE read in your useful Journal the Observations of Dr. Hull, upon a singular case of disease in the bowels, which was published by me in the second volume of the Medical and Chirurgical Transactions. I feel no difficulty in acknowledging that his explanation of the case is more satisfactory than my own; and as it is supported by an examination of the diseased parts after death, in a case which bears a strong analogy to mine, I think it highly probable that his is the true explanation.

If, therefore, the volume in which my paper is published shall ever come to a second edition, I shall take care to correct my mistake. I am, &c.

Great Windmill Street, Jan. 11, 1802. M. BAILLIE.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

THE following detail of a caſe of Catalepſy, which fell under my care a few years ago, may not be unworthy of a place in your uſeful Journal; and I ſhall alſo trouble you with two caſes of Diabetes. I am, &c.

Norwich, Dec. 10, 1802. R. LUBBOCK.

J. B. ætat. 23, a ſhopkeeper by trade, after experiencing ſome ſlight depreſſion of mind, and without any ſtrongly marked bodily weakneſs, was ſeized with ſlight and very ſhort attacks of inſenſibility; at firſt theſe attacks were not frequent, happening at uncertain periods, or perhaps only once in two or three days. In a ſhort time, however, they became more frequent, and he was ſeized ſeveral times in the day, ſo as to diſable him, in a great degree, for the execution of his duties as a ſhopman. At times he would be attacked when attending to his employment in the ſhop, and it made no difference whether he was in an erect or ſitting poſture, as he preſerved the ſame attitude until he regained his ſenſibility. At other times he would be taken during his meals, ſitting in the poſition in which the acceſſion aſſailed him, for ſome minutes, and carrying more the appearance of a ſtatue than of a living being.

In conſequence of the frequency of theſe attacks his employer wiſhed him to go home to his friends, and to follow ſome medical advice. In this ſituation I was deſired to ſee him, and found him contending with acceſſions that appeared to me truly of a cataleptic nature; they occurred frequently in the day, and equally whether he was in a ſitting, an erect, or reclined poſture. At one time, if he walked with his father a few hundred yards, he would be frequently ſtopped by an attack, when he would remain for a minute or two in an erect poſture, and ſtanding firmly upon his legs. And in my viſits I have occaſionally ſeen him in bed, and reclined, and upon the ſtretching out of either arm for the attainment of any object, he would be ſuddenly ſeized, and remain in a ſtate of inſenſibility for ſome time, with the arm ſtretched out, and forming a conſiderable angle with the trunk of the body. But the moſt extraordinary occurrence of this ſingular diſeaſe took place when he was viſiting ſome friends a few miles from his father's. One day having it only in contemplation to write a letter to his father, and without being in the act of writing, he

was ſuddenly ſeized with a fit; and ſome by-ſtanders obſerving a motion in his right hand and fingers, reſembling that of a perſon in the act of writing, a pen with ink was placed between the fingers, and ſome paper being alſo conveniently placed, he regularly began a letter to his father, of which he wrote two or three lines, containing the common and uſual expreſſions of duty, and ſaying that he hoped he ſhould regain his health. The lines ſo written were ſhewn to me; and one day, during my viſit, obſerving a ſimilar motion of the hand, I witneſſed the ſame phenomenon of his writing about two lines of a ſimilar letter.

In addition to the above mentioned particulars of this intereſting malady, I wiſh to ſay that it continued for five or ſix weeks, during which time volatiles, ether, the fœtid gums, opium, ſome doſes of calomel, with the preparations of zinc and copper, were tried in vain; and medicine ſeemed to have little or no influence in reſtraining the paroxyſms, until he took half a drachm of cinchona and the ſame quantity of valerian in ſubſtance, every four hours, when they gradually decreaſed, and left him entirely, and in the enjoyment of health. This patient had a ſimilar attack ſome months afterwards, although in a ſlighter degree, and which ſoon yielded to the ſame medicines.

While I am ſpeaking upon the ſubject of Catalepſy, I wiſh to add, that a few years ago I was called to viſit a young gentleman, of a delicate conſtitution, labouring under a very ſingular acceſſion, and which I could claſs under no other head than that of Catalepſy. This patient, without any adequate or aſſignable cauſe, would fall into a violent ſtate of laughter, which would continue, to the ſevere diſtreſs of his friends, who were apprehenſive he might expire from fatigue, for ſix or eight hours without remiſſion, and in which ſtate he ſeemed ſolely attentive to the buſineſs of laughing, being regardleſs of the obſervations, the wiſhes, or the commands of thoſe around him. This attack, I am aware, may by ſome be deemed hyſterical, but from the torpor of mind to every other particular but the act of laughing, I feel diſpoſed to think it ſomething cataleptic. This patient, after ſome time, regained his health from a ſtrengthening plan of medicine.

In a former Number of your Journal, I publiſhed ſome remarks upon the nature of Diabetes Mellitus; the treatment of which diſeaſe, although conducted upon the moſt approved method, appeared to be unſucceſsful. I beg now to ſay, that two of the three patients mentioned have been dead for ſeveral months; and from the ſtate in which the third remained when

I heard

I heard of him, through a medical friend, ſome months ago, it is probable he is no more. In theſe caſes it appeared that the liquid ingeſta and the urinary diſcharge bore a more direct ratio to each other than has been uſually ſuppoſed by authors, and that the reduction of the quantity of urine followed that of the liquid taken. The ſame remarks I have found to apply to the following caſes, of which I have given a general outline; the one a caſe of Diabetes Mellitus, the other of Diabetes Inſipidus.

John Barker, ætat. 38, a huſbandman, was admitted into the Norfolk and Norwich hoſpital, September 12th, and was found to paſs nearly eleven pints of pale ſtraw-coloured urine in twenty-four hours, ſweet to the taſte, and a pint of which, by evaporation, yielded about half an ounce of ſaccharine extractive matter. Upon keeping an accurate account of the liquid taken and urine diſcharged, it appeared, that after the firſt few days, they were in a near proportion to each other, the urine not exceeding the liquid taken; ſo that if he took four, five, or ſix pints of liquid in twenty-four hours, rather leſs urine was generally paſſed. This patient was permitted to drink beer or any common liquid, and to take alſo the uſual diet of the hoſpital, my object being in a great meaſure to determine the relation that the liquid taken and diſcharged bore to each other. He uſed alkaline medicines, but without any marked effect either upon the quantity or quality of the urine; and the warm bath was tried every other day, for a fortnight, without any advantage. His ſkin was dry, and he was emaciated; he complained of thirſt; his appetite was good, and his bowels regular.

After remaining five weeks in the hoſpital, finding the urine reduced to its natural quantity, not paſſing more than three pints daily, he wiſhed to return home, as he ſaid he could perform ſome light work. His emaciation was ſtationary.

The patient laboring under Diabetes Inſipidus was a tailor by trade, and 34 years of age; he complained of very ſevere thirſt, and paſſed in twenty-four hours from 14 to 18 pints of watery urine, having no ſweetneſs, and a pint of which by evaporation yielded only about one drachm of ſaline extractive matter. His appetite was good, his ſkin was dry, although not wholly unperſpiring; he was an out patient of the hoſpital, and tried various alkaline and acid medicines with preparations of iron without any good effect, he alſo uſed the warm bath every other day, and without advantage, for three weeks. At length giving, for ſome time, and till the gums were ſlightly affected, ſix grains of pilul. hydrargyri every night, his thirſt gradually abated, the urine was reduced to its natural ſtate and quantity,

and he regained his usual health. In this case the liquid taken and urine passed were in due proportion to each other.

From what I have seen of Diabetes Mellitus, I am led to think that a reduction of the urine taking place from diminishing the liquid taken, has induced patients, not unfrequently, to think themselves getting well, although the diabetic macies remains unaltered; and in the same manner, in many instances, it seems probable, that in treating Diabetes, authors have ascribed to the quality of the liquid taken an effect that should more probably be given to a diminution of its quantity. I make this remark, finding that an intelligent correspondent has, in the last number of the Medical Journal, given some cases of this complaint, in which lime water seems to have effected a cure. In these cases no mention being made of the quantity of liquid taken daily, it is probable, while using the lime-water, the sum of the whole liquid taken might be considerably reduced. In remarking upon these cases, I should regret much to appear to doubt either the fidelity or the judgment of your correspondent; but having seen the fatality of this disease, in several instances, and knowing, from an able communication which I have lately received from Mr. Reeve, one of the Presidents of the Royal Medical Society at Edinburgh for the present year, that its fatality has been extensively evinced in the Edinburgh Infirmary, and under the ablest treatment, (for it appears, that of thirteen diabetic patients admitted into that Infirmary in no very long time, twelve have died) I was surprised to find it so readily yielding to so simple a medicine as lime-water. I may also observe, that the event of the case of the nobleman mentioned by Willis does not appear; his complaint was only relieved at times, and perhaps as much from the diminished quantity of liquid taken, as from its quality, whether lime-water or any other liquid. Willis acknowledges that this patient's complaint frequently returned.

Before I conclude, I wish to guard against a source of error in comparing the liquid taken and discharged in Diabetes. In all the cases which I have seen, I should have concluded, that more urine was passed than there was liquid taken, had I been satisfied with comparing the liquid ingesta and egesta, for the first twenty-four hours, or even two or three days, after they fell under my care; for I have constantly found, for a short time, the urine surpassing the limited quantity of liquid taken; but this excess I have always, and I think with reason, attributed to the greater quantity of liquid taken by patients, for the last twenty-four hours, or perhaps two or three days, previous to their adopting any mode of treatment. Thus, in the first case of Diabetes (if it may be so called) mentioned in the last Medical Journal, when it is said that the

the patient paſſed ten pints of urine in one night, although he took only one pint of liquid, I ſhould contend, that this occurrence is no proof that the urine exceeds the liquid taken; for before ſuch concluſion is warranted, it muſt be evident, that it ſhould be aſcertained what liquid had been taken by the patient during the previous twenty-four or perhaps thirty-ſix hours.

In making theſe remarks, I do not mean to deny that there are caſes in which the urine exceeds the liquid taken, for my ingenious correſpondent above-mentioned has remarked, that ſuch ſeems to have been the caſe with ſome patients, lately, in the Edinburgh Infirmary.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

FROM the active and decided part which you have taken in favour of Vaccine Inoculation, you cannot be indifferent to its progreſs; I therefore ſend the following intelligence for inſertion, if you deem it worthy of a place in your uſeful miſcellany.

By a letter received from the Rev. Mr. Pitt, chaplain to the Engliſh Factory at Peterſburgh, I am informed, that vaccine virus, tranſmitted by Dr. Rogers, has at length ſucceeded in producing the happy preventive of the Small-pox in that city. The practitioner who was ſo fortunate as to confer this incalculable benefit on that vaſt empire was Dr. Simpſon, who, as Dr. Rogers is informed, inoculated his own children with ſucceſs.

Dr. Sacco, of Milan,* has ſent to Dr. Jenner ſome vaccine matter, originally procured from a cow in Lombardy. With matter from the ſame ſource, Dr. Sacco had inoculated above eight thouſand perſons, and was in poſſeſſion of a conſiderable number of facts, to prove its efficacy in reſiſting the Small-pox.

Some of this matter, with which I was favoured by Dr. Jenner, has excited the genuine puſtule; and, in my own practice, and in that of others, is now ſpreading the vaccine preventive in every direction.

Dr. Woodville, by whom this valuable and intereſting preſent from Dr. Sacco was tranſmitted to Dr. Jenner, has alſo been ſo fortunate as to produce the genuine Cow-pox with the virus which he himſelf received from Dr. Sacco.

Theſe

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* See Retroſpect.

Thefe are the firft inftances of the production of the vaccine puftule in England, by foreign matter. Such an event cannot but be a frefh caufe of exultation to every benevolent mind, and particularly to thofe who have exerted themfelves in promoting the new practice; as it will ftimulate other medical men, in other countries, to feek for indigenous matter, and accelerate the propagation of vaccinifm over the whole face of the globe.

It muft afford additional fatisfaction to your readers to learn, that the Cifalpine Government has refolved to render the new inoculation general, throughout all the territories of the Republic.

I lately received a letter from Dr. De Carro, the founder of Vaccine Inoculation on the Continent of Europe, in which he fays, "You know I had the happinefs to be the firft phyfician on the continent who adopted the new method, and that my own fons were the fubjects whom I took for that experiment. My example was foon followed by innumerable practitioners; and now it would be abfolutely impoffible to name the countries and towns that have adopted it. It would be fhorter, and more accurate to fay, *all Europe.* After very nearly three years of fuccefs, I need not tell you what I think of vaccination: I know of no encomium that can give an adequate idea of that bleffing to mankind.

"Among the extravagant publications againft vaccination, I have lately read one, in which the author, a phyfician at Frankfort, ferioufly endeavours to prove, by prophecies of the Holy Scriptures, and of fome fathers of the church, that it is no lefs than Antichrift!!!—Nothing reflects greater honour on the Jennerian Doctrine than fuch attacks; and nothing fhews more, how difficult, or rather impoffible it is, to refute it properly, than the neceffity of having recourfe to fuch pitiful refources.

"Remember me to Dr. Jenner; no medical man ever excited my admiration and veneration fo much; he is not only great by the magnitude of his difcovery, but he is alfo great by the manner in which he conducted his refearches, by the perfection which he gave to them before he publifhed his work, and by the extreme modefty with which he fpeaks of himfelf."

Dr. De Carro† did me the honour to fend me a copy of his own publication, which has had a rapid fale, and of which a fecond edition, with important additions, is in the prefs. With this he has fent another copy, ftating, that it is for one of the beft Englifh Reviewers, who may think it worth while to give an account of it.

He

† See Retrofpect.

He remarks, that as far as he can judge by periodical publications, it does not seem that, in England, we are well informed of the immense increase of foreign literature on the subject of Vaccine Inoculation.

Your Journal having eminently distinguished itself above all others, by giving the most faithful and regular account of the rise, progress, and present state of the practice, I beg your acceptance of Dr. De Carro's work. It is a tribute due to the ability, candour, and impartiality, which you have displayed on this most interesting occasion.

As you have not relinquished the review of foreign books, I trust you will deem a critique on the work of so celebrated an author, and of so liberal and enlightened a man as Dr. De Carro, not unworthy of a place in your valuable publication.

New Street, Hanover Square, I am, &c.
Jan. 15, 1802. JOHN RING.

Account of Diseases in an Eastern District of London, from the 20th of December, 1801, *to the 20th of January,* 1802.

ACUTE DISEASES.	
Typhus	3
Pneumonia	5
Peripneumonia Notha	7
Cynanche	2
Rheumatismus Acutus	3
CHRONIC DISEASES.	
Tussis	25
Catarrhus	5
Dyspnœa	12
Tussis cum Dyspnœa	11
Asthma	2
Pleurodyne	5
Hæmatemesis	1
Phthisis Pulmonalis	4
Hydrothorax	3
Anasarca	4
Palpitatio	1
Ophthalmia	4
Cephalalgia	3
Hemicrania	2
Paralysis	2
Vomitus	3
Hæmorrhois	2
Dyspepsia	5
Enterodynia	4
Dysuria	1
Chlorosis	7
Amenorrhœa	8
Lumbago	3
Rheumatismus Chronicus	25
PUERPERAL DISEASES.	
Ephemera	3
Menorrhagia Lochialis	4
Dolores Post Partum	3
INFANTILE DISEASES.	
Diarrhœa	6
Febris Mesenterica	1
Pertussis	5
Tinea	3

The state of the weather during the last few weeks has, as it might be expected, been productive of those diseases which have their seat in the cavity of the thorax. Not only did the severity of the cold occur at a very early period, but very quick changes have also taken place, during this period, in the state of the atmosphere. The alternations of freezing and thawing have been very frequent; a material change of temperature has been

been felt within a few hours. Under all theſe circumſtances it is not ſurprizing that the diſeaſes which uſually prevail at this ſeaſon ſhould now make their appearance, nor that their ſymptoms ſhould be conſiderably aggravated. Accordingly, we find that catarrhal affections, coughs, peripneumonies, and other complaints of the cheſt, are very general. The ſeverity of the ſeaſon has proved fatal to a number of thoſe who were far advanced in life, and eſpecially to thoſe who have been for ſome time ſubject to affections of the cheſt at this ſeaſon of the year. In younger ſubjects, inflammations in this cavity have occurred in ſeveral inſtances; theſe have generally yielded to the uſe of leeches and bliſters, cathartics and antimonials.

It appears from the laſt yearly Bill of Mortality, that the number of deaths by the Small-pox was leſs by nearly one thouſand than in the preceding year. Whilſt we are aware that very little dependance is in general to be placed on a report taken in the manner in which that is taken from which the liſt of deaths is formed, yet we muſt acknowledge, with reſpect to the diſeaſe juſt mentioned, that it is not likely to be miſtaken, even by thoſe who are incompetent to give a proper report in other inſtances. May we not hope that the Vaccine Inoculation, as it is more generally adopted, will continue to diminiſh the influence, and finally to produce the total extirpation, of one of the moſt cruel and fatal diſeaſes to which the human frame is ſubject.

The DISEASES of the YEAR 1801.

Abortive and Stillborn	407	Deſpair	1	Miſcarriage	1
Abſceſs	24	Diabetes	1	Mortification	223
Aged	1562	Dropſy	865	Palpitation of the Heart	1
Ague	4	Eaten by Lice	1	Palſy	79
Apoplexy & Suddenly	238	Evil	7	Pleuriſy	22
Aſthma and Phthiſic	427	Fevers of all kinds	2908	Quinſy	5
Bedridden	2	Fiſtula	3	Rheumatiſm	5
Bleeding	14	Flux	10	Scurvy	4
Burſten and Rupture	21	French Pox	14	Small Pox	1461
Cancer	57	Gout	84	Sore Throat	4
Chicken Pox	2	Gravel, Stone, and Stranguary	12	Sores and Ulcers	6
Childbed	170	Grief	3	Spaſm	4
Colds	6	Headmouldſhot, Horſeſhoehead, and Water in the Head	72	Stoppage in the Stom.	6
Colick, Gripes, and Twiſting of the Guts	8	Jaundice	79	Surfeit	1
Conſumption	4695	Jaw Locked	4	Swelling	3
Convulſions	3931	Inflammation	557	Teeth	333
Cough, and Hooping Cough	428	Livergrown	4	Thruſh	28
Cramp	1	Lunatick	127	Vomiting and Looſeneſs	1
Croup	14	Meaſles	136	Worms	9

Chriſtened	Males	9400	In all 17814
	Females	8414	
Buried	Males	9661	In all 19374
	Females	9713	

Decreaſed in the Burials this Year 3694.

Experiments and Obſervations on the Atmoſphere of Marſhes. By ADAM SEYBERT, M. D.

WHEN inquiries which have attracted the attention of a Franklin, a Prieſtly, an Ingenhousz, and many other eminent perſons, without being decided, are undertaken by one whoſe abilities are ſo far inferior as mine, little ſucceſs can be expected. This remark announces the difficulty of the ſubject I am about to inveſtigate; nevertheleſs, I am ſtimulated by the induſtry of my predeceſſors, and if I cannot promiſe much new matter, I hope to be at leaſt able to verify ſome obſervations and perhaps diſprove others; for in proportion as we remove errors we approach nearer to truth.

When we are fully perſuaded, that to live and to breathe are ſynonimous terms, and that the abſolute neceſſity of air to the maintenance of animal life has been fully eſtabliſhed by repeated and well concerted experiments, we need not be ſurpriſed to find many perſons engaged in an examination of the chemical qualities of our atmoſphere: the names of Scheele, Prieſtly, Lavoiſier, Fontana, &c. will for ever make this branch of ſcience reſpectable.

From the earlieſt ages it has been ſuppoſed that the atmoſphere has great influence on the human body in producing diſeaſe, as well as in reſtoring health; hence the accounts of Hippocrates, Sydenham and Huxham. Phyſicians ought always to notice the ſtate of the atmoſphere during the prevalence of epidemic diſeaſes.

Before facts were collected and experiments well performed, the atmoſphere was ſuſpected to differ materally in almoſt every ſituation; but latter experiments have proved that our notions have been erroneous to a great degree.

In a former memoir which I had the honour to read before your ſociety, I paid particular attention to the atmoſphere over the ocean, rivers and neighbouring land, and hope that my experiments have been of ſome ſervice towards the eſtabliſhment of truth; in the preſent eſſay I intend giving an account of ſome experiments which I peformed at different times on the air over marſhes.

A few general remarks reſpecting the common ſtate of our atmoſphere, perhaps become neceſſary, for the ſake of future compariſons.

We no longer believe, for experiments have taught us the contrary, that our atmoſphere is an homogeneous element; the preſent ingenious doctrines of heat have thrown much light upon the ſubject; and with much reaſon ſome philoſophers are induced

duced to believe "that the aeriform ſtate is a modification of bodies, dependent on the degree of temperature, and on the preſſure which theſe bodies undergo!"* This opinion has been extended ſo far as to induce ſome to ſay, "Perhaps alſo metals are contained in the atmoſphere."† Theſe ſentiments do not appear to be merely conjectural, for Chaptal has precipitated mercury from oxygen gas (which was obtained from red precipitate) by means of ice; and the family of Achard, ſuffered ptyaliſm from breathing in an atmoſphere where mercury had been expoſed for ſome time in a ſaucer.

The above opinions, if true, (and I think them highly probable) prepare us to meet many difficulties in the analyſis of the atmoſphere. But all I expect to do is to open the paſſage, and I ſhall leave others to render it more certain; for numerous experiments, and thoſe often repeated, are the only means whereby we can aſcertain truth; and I fear the labours of one man are inſufficient to perform this taſk.

I am not without hopes that others will engage in the inquiry, it is of importance to every citizen, more eſpecially ſince we find that our principal cities are almoſt yearly afflicted with a terrible epidemic, which has been by ſome attributed to the ſtate of the air. Future diſcoveries, it is hoped, will multiply the number of teſts for airs, and thus render the ſubject more eaſy.

Reſpectable chemiſts have determined the component part of our common atmoſphere to be

Oxygen gas - - -	27.
Azotic gas - - -	72.
Carbonic acid gas -	01.

Any deviation from this ſtatement muſt be attributed to local circumſtances.

I ſhall firſt endeavour to determine, whether or not the air of marſhes differs from that of other ſituations:

2. What are the cauſes of the differences which are found to exiſt: And,

3. Make a few obſervations and remarks.

1. March 31ſt, 1798. Air was obtained by agitating ſtagnant water over marſhy grounds; the following experiments were then performed.

a. It burned when a candle was applied to it; the flame was blue: it did not explode when mixed with atmoſpheric air.

b. Agitated

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* Lavoiſier's Elements of Chemiſtry, p. 59.

† Girtanner's Antiphlogiſtiche Chemie, p. 58.

b. Agitated with lime water, a copious white precipitate was formed.

c. Its bulk was confiderably diminifhed by agitating it with lime water.

d. Equal bulks of it and nitrous gas were introduced into my eudiometer tube, and a diminution of $\frac{2}{100}$ of their bulk was perceptible.

Thefe experiments were frequently repeated, and the refults were fimilar to the above-mentioned: they warrant the following inferences:

a. That carbonic acid gas enters largely into the compofition of the air examined.

b. That hydrogen gas is an ingredient in it.

c. That no oxygen gas was prefent: for the fmall abforption which took place, I attribute to the action of the water with which the airs were agitated.

The above experiments were performed on the air, which was obtained immediately as it was difengaged from the marfhy foil; it became neceffary to examine the air fituated at fome diftance above the marfh.

At different times during the fummer of 1798, I collected air from above marfhy grounds: the following experiments were performed on it.

a. When agitated with lime water, it afforded a precipitate, which was not fo abundant as in the former experiments.

b. Mixed with nitrous air, its bulk was diminifhed to almoft as great a degree as the air in the yard of my lodgings.

c. Either pure or mixed with atmofpheric air, it did not burn or explode when a candle was applied to it.

Hence it appears that the air obtained at the height of feveral feet above marfhes,

1. Contains little or no hydrogen gas.

2. That the proportion of carbonic acid gas is pretty confiderable; and,

3. What is of great confequence to be obferved, a large quantity of oxygen gas enters into its compofition.

The laft mentioned facts induce us to believe, that the air *above* marfhes is not confiderably different in its properties from the common atmofphere in other fituations, where animals refpire with eafe and enjoy perfect health, except the proportion of carbonic acid gas being greater; and this I am induced to believe diminifhes in quantity as we afcend: for facts related by travellers who have vifited the Grotto del Cane and other fimilar places, prove that the gravity of this elaftic fluid permits it to rife but to an inconfiderable height.

After having proved that certain qualities do exift in the air

over marshes, which are different from those possessed by the atmosphere in other situations, we must next attend to our

Second object, viz. to ascertain what are the circumstances about marshes which produce such effects?

Before we proceed any further, it is of the greatest importance to be satisfied respecting the changes which may be produced on common atmospheric air, when subjected to the action of the soil of marshes.

At different times in the months of September and the commencement of October 1796, I exposed atmospheric air to the action of mud, which I obtained from marshes below the city. The same was done at different times in the months of April and July, 1798. The experiments were exposed to the temperature of the atmosphere. The results from the different experiments were similar. The air was exposed to the action of the mud which was contained in a tumbler, by means of an inverted glass jar, in a bason containing a small quantity of water. The following changes were noticed.

1. The air contained in the jar became much diminished in bulk, as was proved by the water rising into the jar.

2. The air, thus acted upon, when agitated with lime water, afforded a copious white precipitate and became diminished in bulk.

3. In some of the jars, were suspended papers, stained blue with litmus and yellow with turmeric; the blue received a reddish tinge and the yellow remained unaltered; the red was again changed to a blue by exposure to the vapour issuing from a bottle containing ammoniac.

4. The air thus altered by the mud, when mixed with nitrous gas in the eudiometer tube, was in every instance found to have lost in point of purity; sometimes no diminution of bulk whatever took place.

The following circumstances seemed to influence the last mentioned experiments. 1st. Temperature. 2d. The length of time during which they were continued. And 3d. The proportion which the mud and air bore to each other; the surface of the mud being more or less extensive, seemed also to have its effects.

The air thus affected by the action of the mud would in no instance burn or explode, when a candle was applied to it; hence it contained but a small quantity of hydrogen gas.

This last mentioned fact induced me to engage in an essay to determine the origin of the hydrogen gas which abounds in the *air obtained by agitant stagnant waters.*

It is necessary to be observed, that in the above experiments with mud, but a small proportion of water was added to it in the

the tumbler, the quantity was juſt ſufficient to promote putrefaction. I am of opinion that the hydrogen gas is afforded by a decompoſition of the ſtagnant water, effected by the putrefaction of the dead animal and vegetable ſubſtances, which enter largely into the compoſition of the ſoil of marſhes. I was induced to form this opinion, becauſe, firſt, pure water is a compound of but two elements, conſequently the affinity cannot be broken but by the action of a third ſubſtance. And ſecondly, we have no experiments which prove that pure water has undergone ſpontaneous decompoſition. My ideas are confirmed by a fact well known to all ſeamen, viz. when a candle is applied to the bung hole of a caſk containing *river water*, which had been for ſome time cloſely ſtopped, an elaſtic fluid eſcapes, which will inflame, and appears in all reſpects ſimilar to hydrogen gas obtained by other means.

After forming the above conjectures, I determined to perform a few experiments which might tend to confirm or diſprove my opinion. With this view, mud and water, with a very ſmall portion of atmoſpheric air, were at different times confined in bottles cloſely ſtopped and inverted over water: in ſome inſtances the experiment was continued during 20 and 30 days. They were ſubjected to the temperature of the atmoſphere. During the progreſs of the experiments, I perceived that an elaſtic fluid was diſengaged from the materials contained in the bottles, and that the water was evidently diminiſhed in bulk. The elaſtic fluid generated during theſe experiments, 1ſt, inſtantly formed a copious white precipitate when agitated with lime water; 2dly, it burned, when the flame of a candle was applied to it, and poſſeſſed the other properties which are common to air obtained by agitating ſtagnant waters over marſhes.

Theſe facts are deciſive to me on the ſubject, and confirm the above conjectures reſpecting the origin of the hydrogen gas diſengaged from marſhy grounds. It is neceſſary to remark, that ſome danger attends theſe laſt experiments; for a large bottle which was cloſed by a ground ſtopper, was broken on the 25th day of the experiment, by an expanſion of the contained elaſtic fluid: the pieces, which were large, were thrown to the diſtance of 20 feet, and a report was heard louder than that from the firing of a muſket. In general, the bottles had corks faſtened by means of ſtrings bound round them: as ſoon as I cut the ſtrings, the corks were forced from the necks of the bottles with conſiderable violence.

The above experiments teach us that mud vitiates the atmoſphere in a very powerful manner. They alſo enable us to account for the preſence of the elaſtic fluid forming the atmoſphere of

of marſhes. It appears, that the carbone of the mud unites with the oxygen of the decompoſed water, and forms the carbonic acid gas, whilſt the hydrogen gas is ſet at liberty. Theſe are truths not to be invalidated by gratuitous aſſertions, ſince their baſis is experiment.

It may be aſked, if mud ſeizes oxygen gas with the avidity ſtated, how comes it that eudiometrical experiments prove the air over marſhes to be nearly, if not quite, of the ſame degree of purity as that of other ſituations?

At firſt an anſwer to this important queſtion may ſeem difficult; but ſome examination of the circumſtances attending the ſituation of marſhes, enables us to account for it in a very ſatisfactory manner. It is to be remarked that in my trials with mud, the air was confined under glaſs veſſels over water, conſequently no circumſtances from without could have any influence on the experiments. The air over marſhy ſituations is very different, it poſſeſſes all the advantages of ventilation, &c. in common with the atmoſphere. Beſides theſe circumſtances, a large quantity of oxygen gas is afforded by the living vegetables which ſurround them in abundance. We may alſo obſerve, that frequently large ponds of water are found in their neighbourhood, and that often rivers are at no great diſtance from them: may not therefore a quantity of oxygen gas be diſengaged from theſe waters by the action of the ſun? Experiments are related by reputable authors, wherein water has been decompoſed by the action of the ſun's rays; of this more hereafter.

That the atmoſphere of marſhes, therefore, differs in certain circumſtances from that of other ſituations, and that the ſoil has conſiderable effect in altering the air of the atmoſphere, I think, cannot be doubted. Let us therefore endeavour to diſcover the particular local cauſes which give riſe to theſe variations.

I have before hinted that the putrefaction of the animal and vegetable matters upon the ſoil of marſhes, was the great cauſe of the changes obſerved to exiſt: for every ſpecies of ſoil will not operate in the manner alluded to.

That the cauſe is in the putrefaction of theſe matters, and that this ſtate is abſolutely neceſſary to thoſe changes, I infer from the following circumſtance; marſhes have no noxious influence during the winter ſeaſon. They cauſe diſeaſe when the circumſtances are preſent which promote putrefaction; as a proper degree of heat, a due quantity of moiſture, and the contact of atmoſpheric air or ſubſtances capable of affording oxygen, as *water*. That a certain degree of moiſture is neceſſary, appears evident from White's experiments, related in the Philoſophical Tranſactions:

Transactions: he says, "a certain degree of moisture seems necessary to produce the bad effects of marshes; for mud when perfectly dry did not alter the air." He might have added, that too much fluidity will likewise prevent their bad consequences, which is proved by the neighbourhood being healthy when they are overflowed. An overflow of water may operate by preventing the powerful effects of the sun. Experience teaches us, that their bad effects are discontinued when they become dry. Covering them with clay and other substances not liable to putrefaction, destroys their bad effects, so does cultivation, frost, &c.

Living trees being planted in their neighbourhood renders the situation more healthy, by absorbing the gas exhaled during putrefaction and affording oxygen gas.

White's experiments prove, "1st. During sixteen hours, air confined in a phial over water did not suffer a change. 2dly. Pure clay moistened did not alter the purity of the air. 3dly. Sand moistened did not change the purity of the air. But 4th. *Mud (which consists of earths intimately mixed with dead animal and vegetable substances) rendered the air very impure, as I proved by the experiments which I performed.*

The following reflections occurred to me some time since, and are copied from my note book.

To arrive at any certain knowledge respecting the manner by which marshes can be supposed to affect the atmosphere, we must investigate their composition.

They seem to consist of,

1st. More or less water. 2dly. Different proportions of *dead* animal and vegetable matters. And 3dly, The earthy substances composing the original soil.

Animals and vegetables, when they have suffered death, are subject to the laws which govern inanimate matters in general, and they are liable to the various changes produced by chemical mixture and the laws of chemical affinity: they are acted upon by the powerful agents of nature, and thus suffer decomposition, and form new combinations.

All chymists acknowledge the analysis of animal and vegetable substances to be imperfect. Lavoisier has paid particular attention to the subject. He performed numerous and accurate experiments to determine their composition, and notices in a particular manner the results they afford during their putrefaction. According to him, they consist chiefly of hydrogen and oxygen, combined with carbone: these substances, he says, are found in all vegetables, and none exist without them. Animal substances contain more hydrogen and azote than vegetables do, they also have carbone as a constituent part of their composition: some of both classes contain sulphur and phosphorus.

The

The above are the principles which I ſuppoſe are liable to be acted upon, and thus produce the effects we are about to conſider.

Before we can underſtand the changes to which the above ſubſtances are liable, we muſt take into conſideration, that our atmoſphere is compoſed of the azotic and oxygen gaſes, and a ſmall portion of carbonic acid gas: many view this laſt as adventitious and by no means neceſſary.

Heat, moiſture, the contact of atmoſpheric air and reſt, we know are circumſtances attendant on marſhy ſituations during the unhealthy ſeaſons.

A priori, we might be induced to believe that the following phenomenon would take place, under the above circumſtances.

1. That hydrogen gas would be diſengaged. 2. That the oxygen combining with the carbone would form the carbonic acid gas. 3. That azote would unite with a portion of hydrogen and thus produce ammoniac; whilſt another portion of it would, during its combination with oxygen, form the nitric acid. And 4th. That when ſulphur or phoſphorus were preſent, they with hydrogen would form the ſulphurated and phoſphorated hydrogen gaſes.

We ſhall now endeavour to diſcover whether or not theſe elaſtic fluids enter into the compoſition of the atmoſphere of marſhes.

1. Hydrogen gas. Doctor Franklin has long ſince demonſtrated the production of this elaſtic fluid in marſhy ſituations. Ingenhouſz and others have confirmed the truth of his experiments and obſervations.

My experiments convince me that it is produced in a conſiderable quantity, and that it may be eaſily procured by agitating ſtagnant waters over marſhes. It is alſo evident that this gas is in a ſtate of mixture with the carbonic acid gas.

Although we are certain that a large quantity of hydrogen gas is diſengaged from marſhy grounds, we muſt neverthele's conclude that it bears but an inconſiderable proportion to the atmoſphere at large; for we find that the air immediately above marſhes will not explode upon the approach of a candle: indeed, from its levity, we might ſuppoſe that it occupies the inferior ſtrata of the atmoſphere but for a ſhort time.

2. Carbonic acid gas. That this elaſtic fluid enters largely into the compoſition of the atmoſphere of marſhes, is eaſily proved by agitating it with lime water.

3. Ammoniacal gas. The production of this gas during putrefaction is proved beyond doubt; therefore that it ſhould exiſt in the atmoſphere of marſhes ſeems at leaſt probable, indeed many have inferred conſiderable effects from its preſence; but as they

they did not detect it by any test with which we are acquainted, their opinion is entirely hypothetical.

The following are the results of the means I employed to discover whether ammoniacal gas is present in the atmosphere of marshes. 1. No white clouds appeared, when muriatic acid gas was mixed with air obtained by agitating stagnant waters. 2. Slips of paper stained yellow by turmeric, were suspended in a bottle containing mud and atmospheric air, it remained unchanged; whereas those stained with litmus received a reddish tinge. 3. I never could perceive the odour peculiar to this alkali, when I visited marshes.

The above experiments caused me to doubt the presence of this elastic fluid in the atmosphere of marshes. I was confirmed in this opinion by the following circumstances: 1st. Ammoniac combines readily with water: it is impossible to procure ammoniacal gas over water; therefore we are to suppose that if this fluid is produced it is immediately absorbed by the water of the marsh. 2dly. Carbonic acid gas is abundant in the atmosphere of marshes. By experiment, I ascertained that this acid and ammoniacal gas were very prone to unite and form the carbonat of ammoniac. The experiment was performed in a glass tube over mercury: as soon as the two elastic fluids came in contact, an absorption took place and the bulk of them was considerably diminished: at the same time the sides of the tube were encrusted with a white matter, which possessed all the properties of the carbonat of ammoniac. If such are the phenomena of these experiments, why will not similar effects take place in marshy situations?

4. Nitric acid. The experiments and observations of Thouvenel and others, have long since demonstrated the production of this acid during putrefaction. If it is formed in marshy situations, its presence cannot be proved in their atmosphere, and I am inclined to believe that it is immediately absorbed by the neighbouring waters.

5. Sulphurated and phosphorated hydrogen gases. If these elastic fluids consist of hydrogen gas, holding sulphur and phosphorus in solution, it seems probable that they should be generated during the putrefaction of such matters as contain them as constituent elements. Although Chaptal in his Memoirs de Chimie, p. 141, observes: "Que la boue noire, degagèe de tout vègètal, ne donnoit plus d'air inflammable mais rèpandoit une odeur de foie de soufre." Still he relates no experiment whereby he detected its presence in the atmosphere of marshes. Its ready absorption by water; marsh air when agitated with a solution of the acetite of lead producing no change in it; silver not tarnishing sooner in these than in other moist situations; and

and the air poſſeſſing no peculiar ſmell, are all facts which tend to convince me that it does not exiſt; moreover Kirwan ſays, that hepatic gas united with nitrous air will depoſit ſulphur. I agitated marſh air and nitrous air together in a glaſs tube and no ſuch phenomenon was noticed.

6. Azotic gas. If you burn candles in the air of marſhes, until all the oxygen be abſorbed, and then agitate the remaining air with lime water, ſo as to abſorb the carbonic acid, an elaſtic fluid ſtill remains which poſſeſſes the properties of azotic gas.

7th and laſtly. Oxygen gas. A variety of facts prove that oxygen gas is a principal ingredient in the atmoſphere of marſhes. 1ſt, Candles burn therein with the ſame luſtre as in other ſituations. 2. Animals breathe with equal eaſe as in other places. 3. Eudiometrical experiments prove that it forms as great a proportion here as in other atmoſpheres which are reckoned more healthy.

Auguſt 4th and 5th, 1796—July 8th and 10th, 1798—I collected air from over marſhy grounds to the ſouth and north of Philadelphia; when tried with the Eudiometer, they always proved as pure as the air in the yard of my lodgings. Chaptal in his Memoirs de Chimie, p. 141, aſſerts that the air over the ponds, which border on the Mediterranean ſea (the neighbourhood of which is equally marſhy if not more ſo than the neck formed by the junction of Schuylkill and the Delaware, as I convinced myſelf during my reſidence at Montpellier in the years 1795 and 1796) was equally pure with that of Montpellier, tried the ſame day. When I aſſert that the atmoſphere of marſhes is equally pure with that of other ſituations, I mean that it contains as large a proportion of oxygen gas as ſuch other atmoſpheres do. I do not by any means intend to be underſtood that it is free from foreign mixtures.

I have acknowledged that putrefaction is going on in marſhy places, and likewiſe admit that this proceſs deſtroys the purity of the atmoſphere by abſorbing its oxygen; therefore it may ſeem difficult to admit the *abſolute purity* of the air being equal here to that of other places. People being able to breathe with eaſe over marſhy grounds, is ſufficient proof that the oxygen gas there is adequate to ſupport life. I ſhall now attempt to account for the purity of the air of marſhes as follows. Sennebier has proved by numerous experiments, that living vegetables placed in an atmoſphere of carbonic acid gas or in water ſaturated with this air, expoſed to the action of the ſun, thrive and grow very rapidly: during the experiments the carbonic acid is deſtroyed and oxygen gas is diſengaged. In addition to theſe experiments, Ingenhouſz has taught us that the aquatic plants,

plants, particularly ſuch as grow in the neighbourhood of marſhes, poſſeſs the power above ſtated to a ſupriſing degree; ſee Experiences ſur les Vègetàux, Tom. 2. p. 401. Theſe facts, when properly conſidered and connected with the remarks I made when ſpeaking of the effects of mud on the atmoſphere, I think are ſufficient to account for the phenomenon, which at firſt ſeemed at leaſt doubtful.

The above view of this difficult ſubject will perhaps in ſome meaſure alter our opinions reſpecting the utility of marſhes. Heretofore mankind ſeem to have viewed their exiſtence as noxious to them, and unneceſſary to their happineſs. I confeſs my former opinion reſpecting them coincided perfectly with that of the majority, but at preſent my ideas are very different: I conſider them as very neceſſary to keep the atmoſphere in a *proper degree* of purity, for it is not only the impure atmoſphere which kills animals, but the too pure alſo; and an ingenious philoſopher has well obſerved, that animals live too faſt in atmoſpheres overcharged with oxygen gas. They appear to me to have been inſtituted by the Author of Nature, in order to operate againſt the powers which vegetables and other cauſes poſſeſs of purifying the atmoſphere, ſo that the oxygen may exiſt in a proper proportion, fit to ſupport animal life and combuſtion. I am of opinion, that ere long marſhes will be looked upon by mankind as gifts from Heaven to prolong the life and happineſs of the greateſt portion of the animal kingdom. Perhaps it was originally intended that they ſhould remain uninhabited, and that their only uſe ſhould be that of correcting the too pure atmoſpheres. Although their immediate inhabitants ſuffer diſeaſe from them, ſtill but a ſmall portion of the human race chooſe marſhy ſituations for their reſidence.

After I had read the above before the ſociety, a friend in converſation with me, objected to the operation of marſhes on the atmoſphere being intended to prevent a ſuperabundance of oxygen gas; he obſerved that this effect would be fully accompliſhed by the ordinary combuſtion and the reſpiration of animals. Upon reflection, his objections gave riſe to new confirmations of what I aſſerted: I remarked to him, that very extenſive tracts of country were ſufficiently warm without fires; that in theſe places nature gave uncommon powers to vegetable action, but at the ſame time ordained, that in theſe very ſituations marſhes ſhould be moſt abundant. If we view moſt ſouthern countries, I believe the above facts will be found to exiſt very generally. A further beautiful demonſtration of my propoſition may be adduced from a well known fact, that when vegetable life becomes paralized in the winter ſeaſon the operation of marſhes is then unneceſſary, and is likewiſe ſuſpended by the ſame cauſes, viz. froſt, &c.

Q 2 *Obſervations*

Obſervations on the Modus Operandi of Opium.

[Continued from Vol. VI. p. 478—486.]

IT has been long known that Opium, when applied externally, is abſorbed by the lymphatics, and conveyed by the heart and arterial ſyſtem to the brain and every part of the body.

So long ago as the year 1754, Dr. Monro had found, by experiments, that a ſolution of Opium applied to the ſkin of frogs, rendered them motionleſs, and killed them in the ſame way as when applied to their interior parts.* He obſerves, (p. 306) " The effects are, however, more ſpeedy, where the doſe is equal, when the opium is applied inwardly than when applied outwardly, as might have been preſumed from the greater ſenſibility and delicacy of the inward organs."

" But the moſt important point concerning the action of opium on living animals eſtabliſhed by this author, (Dr. Monro) is its being received into the ſyſtem by means of the abſorbents; which, as far as I can judge, he proves inconteſtibly by ſhewing that the effects of opium applied to a limb, after all communication between it and the reſt of the body, by means of the nerves, is cut off, are ſoon felt in every part of the ſyſtem."

" We may indeed perceive, (ſays Dr. M.) that the effects of all the foregoing medicines (among which opium is included) when they are applied to the ſound outer ſurface of the body, are chiefly owing to their abſorption, mixture, and conveyance with the blood, ſince they operate as violently, and nearly as ſoon, when the nerves of the part to which they are applied are cut, as when they are entire."

I ſhall quote but one more obſervation from Dr. Monro's paper. It deſerves particular attention; namely, That " the effects of an over-doſe of opium, as well as of ſome other medicines, on frogs, are analagous to their effects on men and quadrupeds. †

" In other caſes when it (opium) is applied to the ſkin, whether it penetrates by inorganic pores to the viſcera of the abdomen, or if it only acts on the teguments, and particularly on the muſcles conſenting with the viſcera, we would not confidently determine; but we have certainly found the external application

* Eſſays and Obſervations Phyſical and Literary of the Philoſophical Society of Edinburgh, Vol. III. p. 297.

† An Experimental Eſſay on the Manner in which Opium acts on the living Animal Body, by Alex. P. Wilſon, M. D. p. 20, 22, 24.

application relieve the pains and ſpaſms of the ſtomach and inteſtines."*

Bergius, in his Materia Medica, aſcribes conſiderable effects to opium when applied to the ſkin. "Opium (ſays he) externe cuti applicatum, agit ſtimulando, in ſimul ſopiendo; ſi diutius ſupra cutem manet, rubedinem vel veſiculas excitat, ubi nimirum cutis tenera eſt opiumque recens."†

Dr. Crumpe expreſſes ſtrong doubts whether opium applied externally be capable of producing theſe effects, and relates an experiment which he ſeems to think deciſive. See Inquiry,‡ p. 27. But he adds, p. 29, 30, "Lind ſays, that in a caſe of Tetanus, an application of opium and camphire to the feet inſtantly removed the ſpaſm, which upon taking off the application immediately returned with its former violence; an effect which was ſeveral times produced by the repeated application of theſe medicines. Moſeley, on the contrary, who quotes this paſſage, aſſerts from his own experience, that opiates externally applied are not of the ſmalleſt utility, either in the prevention or cure of a Tetanus."‖

"Whytt, in caſes of great pain and ſickneſs of ſtomach, where the patient could not bear laudanum internally, experienced the beſt effects from its external application to the belly and region of the ſtomach.§ On mentioning the matter to my friend Dr. Harding, he tells me he has ſeen two very pointed and deciſive inſtances of the efficacy of the tinct. theb. rubbed to the region of the ſtomach, in removing obſtinate vomiting, and allaying the painful and ſpaſmodic affections to which that organ is liable."

Dr. Alexander publiſhed his Theſis, entitled, "De partibus corporis animalis quæ viribus opii parent," at Edinburgh, in 1790. He attempts to ſhew, (contrary to what ſeems aſcertained by ſeveral of Dr. Monro's experiments) that opium is never received into the ſyſtem by means of the abſorbents. I think it neceſſary to mention this attempt, becauſe Dr. Alexander's experiments have been made ſince the publication of Dr. Monro's; but how unſucceſsful it is, muſt appear at firſt view to every perſon who peruſes the Theſis.¶

Could any doubt be entertained of the poſſibility of introducing opium into the ſyſtem by applying it externally, after peruſing

* Cullen's Materia Medica, Vol. II. p. 256.

† Berg. Mat. Med. p. 457.

‡ An Inquiry into the Nature and Properties of Opium. By S. Crumpe, M. D. p. 27.

‖ On Tropical Diſeaſes, p. 494.

§ Whytt on Nervous Diſorders, c. 7, p. 362.

¶ Wilſon's Eſſay, p. 30—31.

perufing the Treatifes from which the above extracts are taken, the accounts lately publifhed in the Medical and Phyfical Journal for July, 1799, and in feveral of the fucceeding Numbers, could hardly fail, I fhould conceive, of being regarded as indubitable proofs, not only of its being poffible to introduce it in this manner, but of its producing very powerful and falutary effects, when thus applied, and in a very fhort time after its application.

Indeed, what good reafon can be given why opium may not be introduced by this mode of application as well as mercury, camphire, ardent fpirits, &c.

Without ftopping to adduce additional proofs, which I am perfuaded would now be deemed fuperfluous, I fhall go on to make a few remarks on the modus operandi of opium generally.

This is a fubject which cannot be reflected upon without folicitude. If we confult thofe authors who have favoured the public with their fentiments upon it, we fhall be ftruck with aftonifhment at the great diverfity of opinion which prevails, even amongft the ableft modern writers, who muft be allowed to be the beft authorities on the prefent queftion.

Dr. Cullen ranks opium as a fedative.* Dr. Crumpe maintains, that the medical virtues of opium are the refult of a ftimulant property alone.† Dr. Brown confiders opium as occupying the higheft place in the clafs of ftimulants.‡ Dr. Young is filent as to the modus operandi of opium, but he frequently fpeaks of its exhilarating and cordial qualities. He alfo attributes to it fuch effects as could only proceed from a fedative medicine.§ Dr. Darwin pronounces opium to be a powerful ftimulant.‖

In the firft edition of Hooper's Medical Dictionary, the words Sedatives and Stimulants are thus explained.¶

Thefe

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* Mat. Med. Vol. II. Article Sedantia.

† Inquiry, Paffim.

‡ Elements of Medicine, Vol. I. p. 104, 1ft Edition.—"Next to ftrong drink, and immediately above it, *ftands* mufk; above it, volatile alkali; higher than this, æther; and the higheft of all, as far as experiments have yet reflected light upon the fubject, is opium."

§ A Treatife on Opium. By Geo. Young, M.D. p. 33, 47, 49, 61, and 132.

‖ Zoonomia, Vol. I. p. 80, 81, 364, &c.

¶ "Sedatives. From fedo, to eafe or affuage. Thofe medicines are fo termed, which have the power of diminifhing the animal energy without deftroying life; as *opium*, hyofciamus."

"Stimulants, from ftimulo, to ftir up. Medicines are fo termed which poffefs a power of exciting the animal energy, as wine, volatile alkali, muftard, *opium*, &c." Here opium is faid to poffefs the contrary properties of diminifhing the animal energy, and exciting the animal energy.

These are not the only proofs that might be adduced; they are, however, amply sufficient to demonstrate that the question is still by no means free from obscurity, though it has frequently engaged the attention of men remarkable for talents and medical skill. With such a conviction on my mind, the most prudent step I could take, would probably be to relinquish the task at once; but having proceeded so far, and feeling, as I do, the absolute necessity there is for its being speedily determined, whether opium be a sedative or a stimulant, I shall not be deterred from persevering in the discussion, though my own insufficiency should prove to be the only fact clearly demonstrated.

The subject seems to have been rendered more intricate than it really is, by the advocates for the stimulant doctrine not adhering to a precise definition of the terms, stimulants and sedatives.

Another inaccuracy they seem to have fallen into, is, their taking it for granted that wine and alcohol are stimulants, and reasoning upon that supposition, though this is also a disputed point; and Dr. Cullen, after a close investigation, places them in the class of narcotic sedatives.* The following is an example of this mode of reasoning: "The stimulant powers of opium are, indeed, in numerous instances so remarkable, that we find many writers comparing its mode of action and effects to those of substances *universally* reckoned powerful stimulants, as *wine*, *alcohol*, and the whole class of cordials, &c."†

The

* "It is, however, as containing alcohol, that wines are to be considered as medicines; and the considering them as such we have reserved for this place, in which I have put them as narcotic sedatives. That alcohol is such, can hardly be doubted; as, when only diluted with water so much that it can be swallowed, it shows the inebriating, intoxicating, and narcotic effects of other sedatives. When taken in small quantity, and much diluted, it does not indeed immediately show its sedative power; but, on the contrary, it may appear as a stimulant, cordial, and exhilarating liquor. As these operations, however, are in common to it with opium and other narcotics, they do not contradict our opinion of its proper sedative nature." Mat. Med. Vol. II. p. 315—16.——Alluding to wine, he says, p. 317, "It is difficult, however, to set the limits between its stimulant and sedative powers; and if the quantity of it be gradually increased, the latter gradually come on, and concurring with the former, produce at first a degree of delirium or ebriety, which is generally of the cheerful kind, and which occupying the mind, excludes all thoughts of care or anxiety; but the same sedative power carried on still further, renders the delirium more considerable, and gives that irregularity and confusion of thought which is the state of intoxication; and at length the sedative power entirely prevailing, the animal functions, both of sense and motion, are gradually weakened, and the person falls asleep."

† Crumpe's Inquiry, p. 97, 98. See also Elements of Med. Vol. I. p. 192, 235, 236.

The following definitions of the terms ſtimulants and narcotic ſedatives, appear to me to be unexceptionable; and I muſt beg to be underſtood to allude to them, whenever I make uſe of either of thoſe terms.

"This then is the general idea of ſtimulants, that they are powers capable of increaſing the mobility, and of exciting the motion of the nervous power." Cullen's Mat. Med. vol. ii. p. 131.

"Sedatives. Theſe are the medicines which diminiſh the ſenſibility and irritability of the ſyſtem, and thereby the motions and powers of motion in it."* Idem, p. 217.

I ſhall firſt endeavour to prove that opium is not a ſtimulant.

That my remarks may be brought into as narrow a compaſs as poſſible, I ſhall confine myſelf in the examination of this queſtion, to the inquiry of Dr. Crumpe; eſpecially as it contains the moſt powerful arguments that have been, or I believe can be, advanced in favour of the ſtimulant properties of opium.

As an apology for the liberty I am about to take, I ſhall beg leave to quote the following paſſage from the work of the ingenious author laſt mentioned.

"That the advancement of any branch of ſcience can never be retarded, either by a free inquiry into principles, however univerſally and implicitly adopted, or by the promulgation of new opinions, however ill founded, if they alſo be impartially examined, are poſitions which will appear, I imagine, upon a little conſideration, ſufficiently evident. For if the former be founded in truth, they will ſecurely ſtand the teſt, and be more fully confirmed by the cloſeſt inveſtigation; if erroneous, no greater benefit can be conferred upon the cauſe of ſcience, than detecting their errors and defects." Introduction, p. 1.

It will readily be acknowledged, that a treatiſe on the nature and properties of opium, would have been defective, had no notice been taken of its effects when applied to the tongue, eye, noſe, urethra, &c. but I by no means ſee the propriety of drawing any general inference, as to the modus operandi of a medicine, *when taken internally*, from the effects it produces as a topical application.† Beſides, it is highly probable, that ſeveral of the articles, claſſed as ſedatives by Dr. Cullen, and others

* The only writer, that I know of, who has denied the exiſtence of ſedatives, is Dr. Brown, Elements of Medicine, vol. i. p. 13, 14, 234, and 235. But I imagine his opinions on this ſubject will hardly gain general aſſent, till clearer proofs are produced than have yet appeared.

† Compare the four firſt experiments, Inq. p. 24—26, with p. 169.

others enumerated in Dr. Darwin's catalogue of Torpentia, would produce pain, a ſenſe of heat and of inflammation when applied to thoſe parts, in as great a degree as opium. That this is the caſe with reſpect to Camphire, appears from the following:

Exp. 1. I introduced a ſmall quantity of a watery ſolution of camphire into my right eye. It immediately occaſioned conſiderable pain, and an effuſion of tears, which continued ſix or eight minutes, and did not go off entirely in leſs than fifteen; the tunica conjunctiva appeared a good deal inflamed.

Exp. 2. I introduced a ſmall quantity of a watery ſolution of opium into my left eye. The pain was very little more ſevere than that which I experienced from the camphire; it ſubſided rather ſooner; the inflammation was not greater than in the firſt trial.

Exp. 3. I introduced a ſmall quantity of a watery ſolution of tobacco into my right eye, which had been ſubjected to the action of the camphire. It immediately occaſioned moſt acute pain, and a copious effuſion of tears, which continued eight or ten minutes.

Exp. 4. I introduced a ſmall quantity of the ſolution of tobacco into my left eye, which had been ſubjected to the action of opium. It did not occaſion nearly ſo much pain as when it was applied to the right eye, but a good deal more than either the opium or camphire had occaſioned.*

The three firſt experiments were repeated upon my houſe pupil without any difference in the reſult, except that the camphire cauſed more pain than the opium; but it is proper to mention that the eye to which the camphire was applied was a little inflamed at the time.

It appears from the firſt and ſecond of the above experiments, that very little can be inferred, in favour of the ſtimulant qualities of opium, from the firſt, ſecond, third, and fourth experiments related in the Inquiry.† The third and

* The ſolutions uſed were of the ſame ſtrength as the ſolution of opium uſed by Dr. Crumpe, namely, in the proportion of one drachm of each article to two ounces of water. It ſhould not be omitted that two drops of ſp. of wine were added to the camphire; but the trituration was continued ſo long, that the whole, or very nearly the whole, muſt have evaporated.

† The evidence produced by Dr. Cullen, in favour of the ſedative powers of camphire, appears to me to be unanſwerable. Mat. Med. vol. ii. p. 293—298.

Dr. Crumpe ſays, (Inq. p. 28,) "The authority however of a late moſt reſpectable character, Dr. Cullen, *whoſe practical remarks ſhould ever be looked up to,* would lead us to conclude, &c."

 fourth

fourth prove, (what was already known) that opium diminiſhes the ſenſibility of the parts to which it is applied.

Dr. Crumpe's fifth experiment, (p. 27) cannot now be admitted as deciding the point for which it was inſtituted.

The following accurate deſcription is given (p. 30, 31) of the effects reſulting from the internal uſe of opium.

"The effects of opium taken internally come next to be examined; and as their great variety requires ſome arrangement, I ſhall conſider it as affecting, 1ſt. the vital; 2d. the natural; and 3d. the animal functions.

From an adequate doſe of opium, the following changes are obſervable in the vital functions. The pulſations of the heart and arteries are firſt rendered quicker, fuller, and ſtronger, and afterwards ſlower than at the time of taking it. With the increaſe of frequency in the pulſe, the heat of the body is generally ſomewhat augmented; the reſpiration is little affected, except a large doſe has been taken; towards the concluſion of the operation of which it becomes ſlow, ſtertorous, and laborious."

Dr. C. then quotes ſeveral paſſages from different authors, to prove that the *primary* operation of opium taken internally, is to accelerate the pulſe, and excite the force of the circulation; and afterwards relates three experiments, which he made to aſcertain this point, and finds it fully confirmed.

It is admitted by all parties, that the *primary* operation of opium, taken internally, is to produce the effects abovementioned; but it is evidently proved, in my opinion, by Dr. Crumpe's experiments, that it acts *ultimately* and *principally* as a ſedative.

As this is a point of great conſequence, I ſhall dwell a little upon it.

The 7th, 8th, 17th, and 19th experiments, are thoſe to which I particularly allude at preſent.

Experiment 7.* "At one o'clock, P. M. I gave to a robuſt healthy young man, whoſe pulſe beat but 44 in a minute, its natural ſtandard, one grain of opium diffuſed in a ſmall quantity of warm water. He had never before taken any of the medicine, and his pulſe was affected in the following manner." †

It continued to beat 44 till 20 minutes had elapſed.

"After 25 minutes had elapſed, there was a manifeſt increaſe as well in the ſtrength and fulneſs, as in the frequency of the

* Inquiry, p. 34—35.

† Dr. Crumpe's ſcale cannot be introduced here conveniently; but the alterations which occurred may be rendered ſufficiently intelligible without it.

the pulse. In an hour this began to diminish, and continued decreasing till near the end of the experiment. A slight heaviness, which came on 55 minutes after he had taken the opium, was the only other effect experienced from it."

But I wish particularly to point out the alteration in the pulse, marked in the scale as having taken place in 90 minutes after the opium had been taken. It appears that the pulse was then only 42, and the same in 100 minutes; in 110 minutes it was only 40, and the same in 120; and did not return to its natural standard, 44, till 135 minutes had elapsed.

The 8th, 17th, and 19th experiments, prove *decisively* the sedative power of opium. The doses taken were larger, and the *primary* and *ultimate* effects are more clearly marked.

Exp. 8, (p. 35) "Forty-five minutes after twelve, P. M. my pulse beating 70 in a minute, I took two grains and a half of opium dissolved in an ounce of water."*

"In 20 minutes perceived a slight warmth, and soon after a degree of moisture on my skin, the fulness of pulse increasing as well as its frequency. In half an hour I found myself, or at least imagined myself, more alert and sprightly than before; *in 40 minutes perceived a pleasing kind of languor gradually increasing; in 90 minutes a dull head-ach; in two hours time the head-ach was much increased, and attended with drowsiness and nausea; in two hours and a half every disagreeable symptom was increased, my pulse 70; took a spoonful of vinegar, which somewhat relieved the nausea; in two hours and three quarters found all the above symptoms still increasing, and attended with slight vertigo, and tremors in my hands; pulse the same as before. In three hours and a half the nausea was considerably augmented, the other symptoms as before, and I at length threw up the contents of my stomach. The head-ach and vertigo were soon after relieved; but I continued in a stupid state for the remainder of the day.*"

Now, if the symptoms just enumerated are to be admitted as proofs that opium is a stimulant, I am clearly and decidedly of opinion, that it ought *never* to be given with a view to its stimulant effects.

Dr. Crumpe's pulse beat 70 when he took the opium, and the scale shews that in 5 minutes after, it beat 74, and continued to do so till 15 minutes had elapsed. At the expiration of 20 minutes, it beat 76; in 25 minutes, it beat 78, and in 30 minutes 80, the highest number it arrived at; in 35 minutes 72; in 40 minutes 70; in 45 minutes it was reduced to 64;

 in

* The scale is omitted here.

in 50 minutes it continued 64; in 55 minutes it beat 66; in 60 minutes 70; in 75 minutes 70; and in 90 minutes 70.

"These experiments, and the authorities above quoted, seem sufficiently to evince, that the *primary* effect of opium on the pulse is to accelerate and render it fuller; and we can only account for the mistake of those who maintain a contrary opinion, by supposing that they neglected to examine the state of the pulse *shortly* after the opium had been taken, attending only to the ultimate changes it underwent;* and this supposition will receive further confirmation from considering, that the only favourer of this opinion, who, as far as I know, has given a particular detail of any experiments in its support, has totally omitted any account of the state of the pulse during the first half hour;† although, as sufficiently appears from those above related, its frequency is, during that period, more augmented than during any other, and indeed in a shorter time than has been generally imagined. After this increased frequency of pulse has continued for some time, it again becomes slower; and this change, as may be observed in the eighth experiment above stated, and as I have remarked in many others of a similar nature, is frequently *very sudden.* The number of pulsations, in a given time after this change has once taken place, continue to diminish, and at length, if the dose has been any way considerable, fall far short of the natural standard of health; the difference in this respect being generally proportioned to the quantity of medicine which has been taken, and the consequent increase of quickness occasioned by it in the heart's pulsations. If the dose has been so great as to induce death, intermissions in the pulse have been generally remarked before that event took place."

"With the increased frequency of pulse, the heat of the body is, as above-mentioned, somewhat augmented; at least, in all my experiments I found this to be the case, if I could judge by my own feelings, and it was sometimes attended with flushings of the face. I could perceive little alteration in the respiration except the dose had been considerable; by such it was in the end rendered somewhat laborious. Those who have

* Has not Dr. C. fallen into a similar mistake, and one which may produce infinitely worse consequences, by having attended so entirely to the *primary* operation of opium, as not to have allowed the *ultimate* operation to have any influence in his decision? This would have appeared less extraordinary, if he had not described the ultimate effects with so much accuracy and impartiality, as he has every where done.

† Bard. Dissertatio inauguralis de opio, Edinburgi edit. An. 1763.

have had an opportunity of remarking the ſymptoms which preceded death, from an over doſe, have obſerved it flow, ſtertorious, and laborious."

"The natural functions are affected by opium in the following manner: The appetite and digeſtion, from unuſually large, or frequently repeated doſes, are generally impaired, and vomiting often induced; the diſcharges from the inteſtines are diminiſhed or ſuppreſſed; ſecretion and excretion are impeded in every part of the ſyſtem, except the ſkin, the diſcharge from which is evidently augmented."

The following obſervations (p. 39, 40) as far as I can judge, militate ſtrongly againſt Dr. C's theory.

"Theſe effects are generally known and acknowledged to reſult from the operation of opium. The eaſtern nations are ſo well convinced, by experience of its powers in *diminiſhing the appetite*, that, in the famine which prevailed in the Eaſt Indies in the year 1770, the wretched ſufferers purchaſed it at exorbitant prices, to allay the cravings of hunger,* and ſmooth the approach to death. That its too free uſe impairs digeſtion and induces vomiting, is well known; but all theſe effects would ſeem only to ariſe from an over-doſe of the medicine, or from its repeated and conſtant exhibition. Its powers in *leſſening the inteſtinal evacuation* need no proof. *That it diminiſhes the ſecretion of the mucus which moiſtens the mouth and fauces, is evident from the thirſt it is productive of*; and that the ſecretion, or, more probably, the excretion of other fluids are impeded by it, is at leaſt probable, the gall and urinary receptacles having been

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* If ſtimulant medicines be indicated in ſuch caſes, why ſhould we not have recourſe to ſuch as do not produce diſagreeable or dangerous conſequences, when taken in moderate quantities; as horſe-radiſh, or muſtard; or the reſinous gums, as guaiacum, myrrh, &c. or to ſome of the aromatics, as cinnamon, pepper, ginger, cardamom ſeeds, ſnake-root, &c. the eſſential oils or oxygen gas. I imagine there are few perſons who are in that unhappy ſituation, that would not prefer any of the above-mentioned ſubſtances to opium, were it likely to have the ſame effect. Is not the evidence in favour of the ſtimulant properties of the articles I have enumerated much more *deciſive* than that in favour of the ſtimulant properties of opium; and do they not produce effects in almoſt every reſpect different and even oppoſite to thoſe produced by opium? *If opium impairs the appetite and digeſtion, induces vomiting, diminiſhes or ſuppreſſes the diſcharges from the inteſtines, and impedes ſecretion and excretion in every part of the ſyſtem except the ſkin*; *and if the articles enumerated above excite and improve the appetite and digeſtion, allay vomiting, increaſe the diſcharges from the inteſtines, promote ſecretion and excretion in every part of the ſyſtem*; theſe circumſtances alone are ſtrong preſumptive proofs (though I think they might with more propriety be called demonſtrative proofs) that they *cannot* belong to the ſame claſs as opium.

been found in thofe animals to whom confiderable dofes were given, replete with their refpective fluids; and the generality of writers agreeing in pointing out thefe as amongft its moft conftant effects."

"In the animal functions, the principal alterations occafioned by opium are the following: (p. 43, 44.)

The hilarity of mind is by degrees augmented, and continues to increafe, if the dofe has been confiderable, until the delirium of intoxication is produced; which, as when refulting from *fpirituous liquors*, is attended in different conftitutions with different fymptoms. It is, however, more generally productive of a pleafant and joyous ftate of mind than the contrary; and it, in many, occafions an increafed *difpofition* to venery. After thefe effects have continued for fome time, *they are fucceeded by others of a very oppofite nature; the mind becomes gradually dull and languid, the body averfe to motion, little affected by cuftomary impreffions, and inclined to fleep. If the dofe has been confiderable, all thefe fymptoms continue to increafe; and tremors, convulfions, vertigo, ftupor, infenfibility, and deprivation of mufcular action appear varioufly complicated, and in various degrees, proportioned to the excefs in the dofe, and peculiarity of conftitution in the fufferer.*"

We meet with more evidence of the fame kind, (p. 45 to 53) but which is ftill liable to the fame objections as have been already made; and great ftrefs is laid on an experiment made by Dr. Ramfay: but here alfo "*the ufual confequences of inability of motion, difturbed fleep, lofs of appetite, and at length convulfions followed.*" (p. 48.)

"In the eaftern countries, (p. 48.) where opium is taken in very large quantities, its enlivening and exhilarating effects are univerfally known and acknowledged." But are not its *ultimate* effects every where the fame?

"The Turks, and other nations, fwallow it in large quantities, when marching to battle, or under any other circumftances which require a mind void of depreffing fears, and infpired with fortitude."

This opinion is a very prevalent one; but here I would appeal to facts, and enquire whether opium does really, in thefe inftances, produce a mind *void of depreffing fears and infpired with fortitude?*

May not fuch an effect, (fuppofing it for a moment really to take place amongft the Turks, and in the eaftern countries alluded to,) be owing in part to their Predeftinarian principles? Their great inferiority, however, when oppofed to European foldiers, cannot arife, *folely*, I fhould imagine, from want of difcipline;

pline: and what can account more ſatisfactorily for that inferiority, than the habitual uſe of a drug ſo debilitating as opium?

"If oppreſſed by cares or misfortunes, they have recourſe to the ſame aſſiſtance; and from its exhilarating powers experience a temporary ſuſpenſion of melancholy and anxiety. In ſhort, like wine and ſpirituous liquors in civilized Europe, it is in theſe countries the ſupport of the coward, the ſolace of the wretched, and the daily ſource of intoxication to the debauchee."

In many of theſe caſes its exhilarating effects may probably be explained in this way. The too frequent conſequences of anxiety and misfortunes are, loſs of ſleep, want of appetite, inactivity, head-ach, &c. followed by increaſed excitement in the brain, and terminating, ſometimes in ſynochus, ſometimes in typhus. The ſoothing and ſalutary effects of opium in ſuch caſes are univerſally known and acknowledged.

The following is, I believe, a juſt deſcription of the conſequences reſulting from the habitual uſe of opium. Inq. p. 51 and 52.

The Baron de Tott is very particular in his account of the effects of opium on the Turks. Speaking of thoſe who give themſelves up to its immoderate uſe, he ſays, "Deſtined to live agreeably only when in a ſort of drunkenneſs, theſe men preſent above all a curious ſpectacle, when they are aſſembled in a part of Conſtantinople called Teriaky Icharchiſſy, the market of opium eaters. It is there that, towards evening, one ſees the lovers of opium arrive by the different ſtreets which terminate at the Solymania, whoſe *pale and melancholy countenances would inſpire only compaſſion*, did not their ſtretched necks, their heads twiſted to the right or left, their back bones crooked, one ſhoulder up to the ears, and a number of other whimſical attitudes, which are the conſequence of this diſorder, preſent the moſt ludicrous and the moſt laughable picture."

I ſhall cloſe my remarks, for the preſent, by relating the reſult of two experiments, which I have lately made.

In my laſt paper, inſerted in the Medical and Phyſical Journal for December, 1801, I expreſſed myſelf as follows.

"Hitherto, my attempts to aſcertain the *primary* operation of opium, applied externally, on the pulſe, have not been ſucceſsful. Patients afflicted with ſpaſmodic or convulſive diſeaſes being uſually timid and apprehenſive, are not favourable ſubjects for the purpoſe; but I have reaſon to believe opium acts more directly and ſimply as a *ſedative*, when applied externally, than when given internally; and I think it is principally, if not entirely, owing to this difference in the modus operandi, that the

the ſuperior advantages of applying it externally, in certain caſes, are to be attributed."

Since that time I have put this intereſting queſtion to the teſt of experiment in the following manner:

They were made upon Mr. J. Hall, my houſe pupil, who is ſixteen years of age, healthy, but not robuſt. I have frequently examined his pulſe, and always found it, except when hurried by exerciſe, fluctuating between 90 and 108. Every circumſtance likely to render the reſult doubtful was carefully avoided. He ſat in an eaſy upright poſture from the commencement to the concluſion of each experiment. The time choſen for the firſt experiment was ſix o'clock in the afternoon, when four hours had elapſed ſince he had taken any kind of nutriment; he had kept ſtill and quiet all the afternoon, and the room was of a moderate temperature. The preparation uſed was the following:

℞. Tinct. opii. drachm. duas. ol. olivar. drachm. tres, vit. ovi. drach. unam. M. which was rubbed on the right leg and top of the foot till the whole was abſorbed; the hand of the perſon who applied it being covered with a ſoft oiled bladder.

Immediately before the experiment commenced his pulſe beat 108, firm and regular, and the following alterations took place in the different functions:

	Min.	Pulſe.	
In	10	88	(when nearly half was rubbed in) p. ſoft.
	20	84	affected with drowſineſs, and complains of being cold; p. ſoft, eyes look heavy.
	30	84	complains of being cold and drowſy, p. ſoft.
	40	84	cold and drowſy; pulſe weaker.
	50	72	feeble and ſmall; countenance pale; coldneſs continues, and is accompanied with diſagreeable ſenſations.
	60	76	weak, and not quite regular; complains of great weakneſs.
	70	90	regular and rather ſtronger.
	90	92	
	120	96	ſtill complains of being cold; and the diſagreeable ſenſations did not go off till after ſupper, when his appetite was better than uſual.
	180	108	The friction was continued 40 minutes.

The above experiment was made Dec. 11, at 6, p. m. and the following on the 13th, at 9, a. m. The preparation uſed was

was the ſame as before, except that it contained one drachm more of the tincture of opium, namely, three drachms. The principal part was rubbed into the left leg and foot, and the remainder into the right. The application was made before he had breakfaſted, or uſed any exerciſe or exertion, and the liniment was made warm before it was applied.

	Min.	Pulſe	
	—	95	immediately before the opium was applied.
In	15	86	ſoft, and eaſily compreſſed.
	30	83	weak and ſoft; complains of being very weak, cold and ſhivery; countenance looks ſickly; ſkin feels cold.
	45	83	ſoft; complains much of being cold all over, but particularly his feet; is ſhivery and very weak; ſpirits depreſſed; feels drowſy; eyes look dull.
	60	90	weak and ſmall; back painful; complains of being affected with a greater degree, and a different kind, of coldneſs, to any he ever experienced before.

I now aſked if he had felt exhilarated at all, either in this or the former experiment? He anſwered, No; and could not help ſmiling at the queſtion, though he looked like one in the cold fit of an intermittent. He now requeſted to have his breakfaſt. I enquired whether he felt hungry? He replied, that he never was ſo hungry in his life. He ate his breakfaſt greedily, and was ſoon relieved from every diſagreeable ſenſation.

On his ſaying he had often been very cold, but had never before found that ſenſation accompanied with ſuch *diſagreeable* ſymptoms, I mentioned the word Horror, and he ſaid it conveyed the beſt idea of what he had felt.

[To be continued.]

Remarks on Mr. Crowfoot's *and* Dr. Langslow's *Statements of a Case of Apoplexy,*† *with incidental Observations on that Disease.*

By G. Wilkinson, *Corresponding Member of the Medical Society of London, Member of the Royal College of Surgeons, and Honorary Member of the Chirurgo Physical Society of Edinburgh.*

To Dr. BRADLEY.

Sir,

THE Statements of Mr. Crowfoot and Dr. Langflow of a cafe, or fuppofed cafe, of Apoplexy, now before the public, appears, in my opinion, to favour more of perfonal animofity than that of a candid and liberal difquifition into the nature of the complaint. Both parties are equally tenacious of their opinions, and thefe feem as diametrically oppofite. But, as the Editors of the Medical Journal juftly obferve, that many of their readers may be divided in their opinions on this important difeafe, and that a knowledge of its true caufes, and the moft rational mode of treatment, cannot be too foon, or too widely diffufed, and that they have fubmitted it to their Readers, with a view of collecting their fentiments — I beg leave, with all due deference and refpect to the generality of profeffional readers, and to Mr. C. and Dr. L. (to neither of whom I am perfonally known) to offer fome few Remarks on the Cafe, as related by both of them; and as opinions ever will vary in difeafes not perfectly afcertained, or clearly underftood, fubmit what are my general fentiments on the nature and treatment of this important and often fatal difeafe, to the animadverfions and obfervations of better judges than myfelf; being fully perfuaded, that whatever new light may be thrown on a fubject that ftill appears involved in doubt and obfcurity, will be thankfully received.

Different as the Statements of this Cafe may appear, it feems however agreed, that it was an Apoplexy, although neither of the gentlemen faw the patient in the fit. When I fay this, I do not mean to difpute its reality, nor diftruft the profeffional abilities of Mr. Primrofe, the gentleman firft called in, who confidered it as fuch. The term Apoplexy was applied to it, and of courfe a difference of opinion not only took place with refpect

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† Vide Medical and Phyfical Journal, No. XXXIV.

respect to the nature of this disease, but likewise the mode of treatment necessary to be adopted. This, as far as it can be collected from both Statements, appears to be the matter in dispute.

In Mr. C's account, it appears that this lady, 59 years of age, was seized immediately after dinner; but neither Mr. C. nor Dr. L. have given us any intimation of the patient's state of health prior to this sudden attack.

Dr. L. however, by way of exultation, has thrown some light on this affair in his answer to Mr. C. at the time of writing it,* by saying that his patient, at that period, was "in more perfect health than she had been for several weeks *previous* to her attack." From hence it must be inferred, that she was then in an *imperfect* state of health before the fit took place. If the digestive organs were in fault, which there is some reason to suppose might be the case; or the food which she had then eaten was improper; or, in other words, she had indulged her appetite too freely in any particular sort of diet, we shall be enabled to develope, in some measure at least, one of the obvious causes of this attack.

In the majority of cases of this sort that usually occur, it will be found, as Doctor Heberden justly observes,† that they chiefly "attack persons who are past the meridian of life, and frequently such as are at least upon the verge of old age. They are often the distempers of persons worn out with cares, and diseases, and time; and seldom of the young, vigorous, and of the subjects of inflammatory disorders."

It will also be found, that they often attack persons after a full meal, and usually those who are accustomed to indulge in free living. It was in cases of this sort, if I understand rightly, that the late worthy Dr. Fothergill (whose mode of treatment Dr. L. has so arrogantly criticised‡) trusted to evacuations by the stomach and intestines. How far he might be justified in restricting the use of the lancet, may not be easy for us to determine. We may naturally suppose a person of Dr. F's turn for observation, reasoned from experience; and as his practice on these occasions might be attended with success, it seems by no means improbable that the cases to which he refers were unattended with characteristic marks of congestion in the brain, otherwise I can hardly suppose he meant to discard bleeding where real marks of plethora presented.

Indeed, the too indiscriminate use of the lancet, adopted in almost

* Vide Medical and Physical Journal, p. 562.
† Medical Transactions, Vol. i. p. 472.
‡ Vide Medical and Physical Journal, p. 562.

almoſt every caſe which had been ſuppoſed to be Apoplexy, particularly in thoſe where there appeared to be a ſuſpenſion or derangement in the vital organs, was, and ſtill is, too apt to be conſidered as ſuch; hence Dr. Heberden ſuſpected,* and after him Dr. F. was of opinion,† that this operation too frequently proved hurtful. I do not, however, infer from this, that the omiſſion of venæſection, or the exhibition of emetics in Apoplexy in general, are by any means ſafe or juſtifiable. In caſes that are ſtrongly marked, it is certainly proper to bleed *ad vires*; and as it is undoubtedly, at all events, hazardous to venture on the uſe of emetics, I ſhould certainly prefer that of evacuating by the inteſtines.

To me, however, it appears probable, though contrary to the notions of Dr. L. from whom I beg leave moſt reſpectfully to diſſent, that this very fit with which the lady was ſeized, was not occaſioned by *extravaſation* of blood, *exudation*, or even effuſion, from the veſſels of the brain. Nothing ſeems more probable, than that it might have been brought on by a large indigeſted meal (which, as Dr. F.‡ obſerves) "diſtending the ſtomach, preſſing upon the aorta deſcendens, obſtructing the free expanſion of the lungs, and thus crowding the arterial ſyſtem of the head with more blood than ought to be there. Indeed, the good effects reſulting from the bleeding ſeems acknowledged, as the lady, after that, became *perfectly ſenſible*. It removed the turgeſcency or diſtention of the veſſels of the brain and lungs, which occaſioned compreſſion of thoſe parts. The treatment afterwards was proper and judicious; the opening medicine preſcribed by Dr. L. had operated prior to the leeches, which were not uſed till the day afterwards. The nauſea and ſickneſs at the ſtomach may, perhaps, be more rationally accounted for, from that part being irritated from the *indigeſtible* full meal than from the brain being primarily affected. Dr. L. certainly took the ſafeſt method by evacuating from the inteſtines; but admitting that the pulſe had become free, regular, and pretty uniform, after the bleeding, which is not noticed by either of the gentlemen, there is no great reaſon to ſuppoſe a gentle emetic might have proved hurtful, more eſpecially if the belly was not conſtipated. Be this as it may, it muſt however be allowed, that emetics on theſe occaſions are certainly hazardous.

Might not the pain, ſtricture, and ſenſe of oppreſſion which ſtill remained even after the operation of the purge, be owing to

* Medical Tranſactions, Vol. i. p. 473.
† Medical Obſervations and Inquiries, Vol. vi. p. 84.
‡ Idem Liber.

to a partial diſtention, or local plethora, of the external veſſels of the head, the immediate alleviation of which had been procured from the uſe of leeches? This, I think, appears probable, as the Doctor acknowledges, notwithſtanding this, that the pulſe was tolerably free.*

Admitting that the mode of treatment adopted by Dr. L. in the caſe before us, as a phyſician, was perfectly right, and what may with ſome little variation, according to circumſtances, be applied generally with advantage in moſt caſes of this ſort; yet viewing him in the light of a Phyſiologiſt, one can hardly ſuppoſe him infallible, when he ſo poſitively aſſerts that this caſe (which at any rate muſt be conſidered as doubtful) proceeded from actual *extravasation*, *exudation*, or *effusion* on the brain, and that ſome one or more of theſe cauſes muſt exiſt as a *sine qua non*, to conſtitute a *real* Apoplexy. Dr. L. muſt pardon me when I aſſert, that very few, if any, converſant in diſſections of this ſort, will join him in that opinion.

Mr. C. when he contended that none of theſe cauſes, he inſiſts, were *absolutely* neceſſary to conſtitute an Apoplexy, which might be produced by compreſſion in the way as has been deſcribed by Dr. F. was certainly not miſtaken; and that this might be the caſe of the patient who is the ſubject in diſpute, is what I am almoſt firmly perſuaded.

Dr. L. ſays, (p. 559) " That all modern authors are *decided* upon this point, that compreſſion is the only cauſe of Apoplexy, and that it is produced by the effuſion of blood, or ſerum, and generally the former." In p. 560, he ſays, " It is his firm belief that no caſe of *real Apoplexy* could poſſibly occur without compreſſion; and that when the ſkull was not injured, the compreſſion was *always* produced either by *extravasation*, *exudation*, or *effusion*, in *every instance*, when it amounted to abſolute abolition of the ſenſes, and that the patient fell to the ground." Dr. Cullen, however, who is a *modern author*, and a favourite of Dr. L. in enumerating the appearances of the diſſection of ſubjects who have died of this diſeaſe, admits, among various other cauſes, that it is occaſioned " by blood accumulated in the blood veſſels of the brain, and diſtending them to ſuch a degree as to compreſs the medullary portion of the ſame."† The ingenious Dr. Kirkland conſiders that ſort of Apoplexy to which Dr. L. alludes, " to be a diſeaſe *sui generis*, ariſing from an internal cauſe, becauſe it has peculiar ſymptoms; it does not always originate in the head, as has been imagined.

* Vide Medical and Phyſical Journal, p. 554.

† Vide Firſt Lines, Vol. III. Art. 1103.

imagined, but alſo in the viſcera of the thorax or abdomen, or both together. It apparently happens to thoſe poſſeſſing a morbid irritability, occaſioned by ſome kind of affection in the brain itſelf, or ſome other part of the brainular ſyſtem."* The ſame gentleman further obſerves thus: "My friend, Mr. Cheſſhire, of Hinkley, a man to be depended upon, and whoſe ſkill in Anatomy enabled him to detect any morbid appearances, tells me he diſſected many who died of Apoplexies in the Middleſex Hoſpital, without being able to diſcover any extravaſation either of blood or ſerum in the ventricles of the brain, or any part of the ſkull." †

The celebrated Morgagni, in his elaborate Work entitled De ſedibus et cauſis Morborum, has noticed the ſame. ‡

Moreover, the illuſtrious and indefatigable Dr. Baillie, who is no leſs remarkable for his accuracy than the modeſty and diffidence with which he advances his opinions on almoſt every occaſion, ſays, "When blood is effuſed into the ſubſtance of the brain, Apoplexy is produced, which is attended with the following ſymptoms, viz. coma, often ſtertorous breathing, a paralyſis commonly of one half of the body, and often convulſive motions; the pulſe is ſlow, full, and often very ſtrong. When the patient is not cut off at once, but lives for ſome time after the attack, the hemiplegia, which is almoſt conſtantly the effect of this diſeaſe, is upon the oppoſite ſide of the body from that of the brain, in which the effuſion of blood has taken place." ‖

From this quotation, it does not appear that Mr. C.- was ſo far miſtaken as Dr. L. has taught us to believe, viz. "that in his judgment, if conſiderable extravaſation had taken place (in the caſe diſputed) the moſt formidable ſymptoms muſt have ariſen;" or, in other words, he muſt mean that more formidable appearances would have remained after the patient had recovered from the fit; and his ſaying, "there was no marks of paralyſis," which certainly was liable to have taken place, had this really been the caſe, appears, notwithſtanding the Doctor's learned commentary, to be not altogether ill founded. Again, in Apoplexies to which pregnant women are ſubject, my worthy preceptor, Dr. Denman, has obſerved, that in the examination of thoſe who have died, "I have never ſeen an inſtance of effuſion of blood in the brain, though the veſſels were extremely turgid; but it is remarkable that, in all, the heart was found uſually

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* Commentary on Apoplectic and Paralytic Effects, p. 17.
† Idem, p. 28.
‡ Vide Epiſt. 60. Art. 10. et ſeq.
‖ Appendix to Morbid Anatomy, p. 161.

usually flaccid, and without a single drop of blood in the auricles or ventricles. *

In the course of my practice, out of four cases of puerperal apoplexies, three of them died. The first, while in the act of parturition, fell back and suddenly expired. The second remained in the fit several hours, but died half an hour after I was called in; she was the patient of another gentleman; venæsection and glyster had been used, but in vain. The third, who previous to her attack laboured under anasarcous swelling of the whole body, hard dry cough, dyspnœa, &c. and who had been bled, and taken pectoral medicines without much benefit, was suddenly seized with violent head-ach, stertorous breathing, frothing at the mouth, spasms of the muscular system, stupor, and locked jaw, remained in this fit (before she quite recovered) sixteen hours, during which period she was bled freely from the arm, dashings of cold water were alternately applied but with little success, and afterwards flannel cloaths wrung out of hot water, constantly applied and repeated to the legs, thighs, and abdomen; acrid glysters were also injected which procured stools, and lastly a blister inter scapulas. She however recovered after the period of time mentioned, and the labour coming on gradually about twenty-four hours after, she was delivered on the 3d or 4th day of twins, one of which was dead. † The fourth patient was attacked in the same way as the last described, with the very same symptoms, but more violent, her jaw being also closed; the same treatment was adopted in all respects, but without success, she died in about four hours from the attack of the fit. These three last patients were in their first pregnancy, the other had born children before.

None of the bodies of these women was I permitted to examine; but as it might have happened, had they been inspected that no extravasation, exudation, or effusion would have been, as appears to be the case with Dr. Denman's patients, yet I will not pretend to deny but their deaths might have occurred from such a cause, as a case of this sort is related by Mr. Williams, of Rugley. ‡

I am however of opinion, that the third patient who recovered, and who is now living with her child, which is nearly four years old, might have had compression on the brain from the turgescency of the vessels, but I never can believe it that there was extravasation.

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* Vide Introduction to the Practice of Midwifery, Vol. II. p. 408.

† This case, which is curious in some respects, is intended for publication in the Medical Transactions and Observations, by Dr. Simmons.

‡ Vide Medical Transactions and Observations, Vol. V. p. 96.

Two other cafes of Apoplexy have occurred to me in women advanced in life; their ages between fifty and fixty years; neither of them indicated the apopleɕtic *diathesis*, that is, they were not corpulent, fhort necked, or bloated, nor were they given to indolence or intemperance; both of them perfons in the lower ftation of life, both were dyfpeptic, and fubject to a fort of nervous irritability, and at times affected with head-ach, coftivenefs, &c. One of them had indulged herfelf in an afternoon with fome friends at a tea-drinking, where fhe freely indulged her appetite in eating heartily of cake kneaded with butter or hogs lard, and afterwards took a glafs of rum; foon after this, fhe was feized with violent head-ach, ficknefs at the ftomach, impeded refpiration, ftrong fpafms, her jaw clofed, and appeared quite fenfelefs: her pulfe felt remarkably flow, weak, and fluttering, and her countenance appeared pale, fo that I did not deem it proper to bleed her. Upon dafhing cold water upon her a few times, in two or three minutes, or lefs, fhe began to revive, vomited the contents of her ftomach, and had an involuntary loofe ftool, which greatly relieved her. A mixture Ex. magn. c. kali ppt. et conf. arom. was prefcribed; this, with fome tonic medicines, foon reftored her to her health. The other patient was feized nearly in the fame way with fimilar fymptoms, but more ftrongly marked, the fpafms being more violent, refpiration more convulfed, her jaw being alfo locked; the pulfe indicating fymptoms of plethora, being remarkably tenfe, I bled her *ad vires*, found her refpiration become more free and eafy; the convulfions appeared to go off, but fhe did not come out of the fit till I had ufed the dafhing of cold water, and then her fenfes began to return, and her jaw opened. Finding fhe had been much fatigued by over watching, want of reft, abftinence, and a little before the period of her attack had drank fome very acid hard ale, which had undoubtedly irritated her ftomach, acting as a fort of poifon, I prefcribed for her a fimilar mixture to that of the former patient, kali tartarizat. inftead of kali ppt. with good effect. It was, however, fome days before this patient recovered; fhe afterwards complained much of pain and heavinefs of her head, and had fymptoms of pyrexia. She took gentle laxatives, afterwards tonic medicines for a week, till fhe mended.

A remarkable ftout man, aged 63 years, pretty mufcular, with a large head, rather fhort neck, and full countenance, whofe occupation was fedentary, at certain times was affected with head-ach, ficknefs, indigeftion, and occafional coftivenefs. As he lived temperately, took the frequent exercife of walking in the open air, and kept his belly open with aloetic pills, &c. no medical advice was fought for till the beginning of June,

June, 1786, when I was consulted. At that time he laboured under great mental anxiety, with a dull heavy pain in his forehead and temples, accompanied with vertigo, depraved appetite, costiveness, pulse full, and somewhat oppressed, with slight nausea, and thirst. The whole of these circumstances being well weighed as indicating an apoplectic *diathesis*, I bled him in the temporal artery, administered an opening medicine, and he afterwards took some doses of James's powders, by which means his head became easier, his thirst abated, and his pulse pretty free, and regular: his mental anxiety, which was of the religious kind, still continuing, he became restless, took but little food, and refused all medical aid for two or three days. Being after this more composed, he consented to take, as he was costive, a purging medicine, Ex infus. sennæ. sal. Glaub. manna et tinct. jallapii, which was administered in the morning. The whole of the forenoon he felt himself sick, with pain in his head and an inclination to vomit. As he attributed his indisposition to the effect of the medicine more than to the increase of his complaint, I was not sent for till he fell back suddenly on the bed he was sitting, being then seized with a violent fit of what appeared to me to be a *real* Apoplexy. I found him totally senseless, the convulsions were strong, the muscular system violently agitated, his breathing stertorous, his jaw quite locked, his eyes staring and wild, with frothing at the mouth; a very copious involuntary discharge by stool had ensued, and his pulse appeared sunk. From all these appearances, and considering that the system had already been lowered from what had been done before, I was of opinion that a fatal extravasation, or effusion, (call it what you will) had taken place on the brain; and his jaw being closed, so that nothing could be given or got down, I deemed his case desperate, and left him. The time past on, and I was sent for to some other patients; but on enquiry long afterwards, found he was still alive and in the fit. It was then about 10 o'clock at night, when I again visited him; he had been upwards of nine hours in this situation, for the fit had come on about half past 12 in the day. Finding his pulse much more firm and somewhat tense, I resolved to bleed him freely, whilst I reflected that it was *possible*, *extravasation*, *exudation*, &c. might not have happened. At any rate no harm could ensue from the attempt. With some difficulty, and a great deal of opposition from those around me, as he lay on his back, still struggling, I bled him in the arm from a large orifice; the blood issued out with freedom, and by the time I had filled a large bason, I found him gradually recovering, his respiration became more calm and easy, the spasms diminished, his jaw became relaxed, and his senses began to return. His pulse now

acting with freedom; as he had had a large evacuation by ſtool, I ventured to preſcribe for him an anod. ſudorific draught; this procured ſome ſleep, with gentle diaphoreſis, ſo that in the morning he appeared quite calm and ſenſible, the pain in his head, &c. being greatly leſſened. By the time a fortnight had elapſed he ſeemed languid and much debilitated; his legs, thighs, face, hands, &c. appearing quite anaſarcous, the ſecretion of urine was much diminiſhed, his ſkin was dry, his belly coſtive, and pulſe weakened. By the uſe of gentle ſaline cordial diuretics, together with mild purges, Ex pulv. crem. tart. c. pulv. arom. in forma elect. pretty ſtrong infuſions ex ſem. ſinapii et rad. raphn. ruſt. the anaſarcous ſwellings went quite off, the ſecretion of urine even returned; and although he was ſupported by wine and jellies, was allowed ſome brandy and water, and ſuch animal food as was eaſy of digeſtion, with the occaſional uſe of tonic medicines, yet the integuments covering the whole of the external parts ſubject to the anaſarcous affection never were reſtored to their tone, but continued looſe and detached from their adheſion to the muſcles.

As he had no cough, or difficulty in reſpiration, &c. it appeared that he was free from affections of the thoracic viſcera; from a deficiency of bile, however, his ſtools, which were ſometimes whitiſh, with high coloured urine, yellowneſs of the ſkin and tunica conjunctiva, and a ſort of hardneſs and enlargement which appeared in the right hypochondrium, the liver appeared evidently diſeaſed. His appetite, however, did not ſeem greatly impaired till the laſt, as he took his food pretty freely, and at times was in good ſpirits. In this way he continued till the middle of October, when he died. He did not keep his bed till about ſix weeks before, but got up and ſat in his chair. No *paralysis*, *stupor*, or any diſeaſe indicating, or rather what might be expected from the effects of the fit to proceed from the brain, took place; extreme debility, and the return of the anaſarca, combined in ſome degree with aſcites, which appeared from the tenſion of the abdomen and ſwelling of the lower extremities, deficiency of the ſecretion of urine, aided by a partial mortification of the podex, nates, and part of the integuments covering the os ſacrum, from the preſſure of lying in bed, haſtened his death, which took place after a period of near four months from his firſt illneſs. Theſe three caſes which we have related, among ſeveral others that I have *noted*, are adduced if not as abſolutely poſitive, may yet be conſidered as preſumptive proofs that apoplexy, or at leaſt ſymptoms of apoplexy, occur independent of *extravasation*, &c. on the brain. Both of the women were, prior to the attack, in an apparent ſtate

state of tolerable health; they shewed no appearance of the apoplectic diathesis, or tendency to such a disease, and have since continued, and are now in a pretty good state of health. The third, or last case described, wherein a tendency to this disease seemed to evince itself from external appearances, and where the fit lasted so many hours, shewed every appearance of this disease in the strongest light; and although it can scarcely be doubted but Dr. L. will deem this case (if not the others) as being induced by *extravasation*, &c. yet, considering that the viscera was even at that period in an incipient state of disease prior to his attack, and no further appearances of Apoplexy, or its consequences, having ever appeared after the accession of his fit, I am of opinion that this Apoplexy proceeded more from a morbid state of the viscera inducing it, than from any primary affection that took place in the brain, and that it was not produced by *extravasation*, &c. No one however pretend to deny the possibility or even probability of extravasated blood, o serum, independent of external injury, producing this disease but whether this may be the cause or effect has been dispute by some;* nor need we doubt that the former (I mean extra vasated blood) is sometimes absorbed by the powers of the sys tem, aided by evacuations, sudorific opiates, &c. but this apprehend will be found to be the case oftener in instances pro ceeding from external violence done to the head by accidents' and this even where no chirurgical operation took place.†.

Admitting, however, that absorption of extravasated blood occasioning apoplectic symptoms, is effected in the first instance; it seems however doubtful that the effusion, which is usually of the serous kind, frequently remains behind. Dr. Baillie remarks that, "Cavities containing a serous fluid are sometimes observed in the substance of the brain. They almost constantly occur in the medullary part of the hemispheres, and are generally lined with a tough substance or membrane. They would appear to be the remains of the cavities formed by extravasated blood, in cases of apoplexy where the patients have not been cut off immediately, but have lived afterwards for some months or years. The extravasated blood would seem in such cases to be dissolved, and taken up by absorption; but the injury is not repaired, and a cavity remains afterwards filled with a serous fluid."* After what has already been said relative to the dif-

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* Vide Kirkland on Apoplexy. Morgagni Epist. 4to. &c.

† Bromfield's Observations in Surgery. Pott's Works, Bell's System of Surgery, &c.

* Appendix to the Morbid Anatomy, p. 151.

ferent opinions of Mr. C. and Dr. L. in this caſe, as profeſſional men, in which it appears that my ſentiments differ widely from the latter gentleman's, it might perhaps have been expected by ſome of my readers, that I ſhould have thrown out ſome new light on this uncertain, difficult, and, at any rate, obſcure diſeaſe; but to this taſk I feel myſelf totally unequal. Neither ſhall I preſume, after what has already been written on this very intereſting ſubject by thoſe illuſtrious characters who have preceded me, and whoſe opportunities of inveſtigating this important diſeaſe have been greatly ſuperior to mine, even to attempt to enter into a diſquiſition of the ſeveral varieties and diſtinctions into which it has been divided; this, beſides leading me into a labyrinth of mere ſyſtematic conjectures, would not in my opinion tend to produce any thing new or intereſting on a ſubject that has not hitherto been done by men of ſuperior abilities; nor ſhall I attempt to purſue Dr. L. any further in his definitions of ſanguineous and ſerous apoplexy, and his animadverſions on the different doctrines and practice of thoſe who have preceded him. Theſe ſhall be left or ſubmitted to the profeſſion at large, who are beſt enabled to judge how far Dr. L. from the ſpecimen he has now exhibited before us of his phyſiological talents, may be qualified to perform what he has promiſed to lay before the public at a future time, and which, I truſt, after what has happened, he will not diſappoint; his words are theſe. "And it is now my determination, from ſeeing the danger of defective diſcrimination in *theſe caſes, the moment I have a little leiſure*, to write upon this ſubject in ſuch plain and intelligible terms, as to enable the *young and moſt inexperienced practitioner*, when called upon in ſuch emergencies, to act with promptneſs and deciſion, and with confidence in himſelf, till more experienced aſſiſtance can be procured, as a few minutes delay, in ſuch inſtances, might prove fatal."* This, it muſt be owned, is a ſplendid promiſe; and if it were not ſubject to the exception of, "the moment I have a little leiſure," I entertain no doubt that *the moſt experienced*, not to mention *inexperienced*, practitioners will acknowledge themſelves highly indebted to him for this happy and unlooked for diſcovery. From the firſt moment of my peruſing theſe different ſtatements, I felt my mind impreſſed with the difficulty of the ſubject in diſpute, with which it is connected, and which it muſt be allowed is certainly of the utmoſt importance, not only by reaſon of its doubtful nature, which has given riſe to different opinions of its cauſe, but that of the various

* Vide Medical and Phyſical Journal, No. xxxiv. p. 561.

various modes of treatment which have been deemed neceſſary to adopt. This, by inducing me to turn over in my mind what had actually fallen under my own immediate obſervation, and comparing it with what has already been advanced on this ſubject by the moſt approved modern writers of the preſent day, (to whom Dr. L. ſeems principally to appeal) together with the hopes and expectations of its ſtill being further elucidated by more able hands, is a principal cauſe of my preſuming thus to obtrude my ſentiments upon the public. To neither of the parties concerned in this diſpute am I known perſonally, nor to their private animoſities do I pay the leaſt regard whatever; and how far I may have acted with candour and impartiality in the diſpute between them, is ſubmited to the conſideration of impartial profeſſional readers. I am, &c.

Sunderland, Dec. 21, 1801. G. WILKINSON.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

I Have to requeſt the favour that you will do me the juſtice to give an early inſertion in your uſeful Journal to the following letter of Your's, &c.

Yarmouth, Jan. 4, 1802. T. GIRDLESTONE, M. D.

I beg leave to contradict a report circulated in the Medical Journal of laſt month by Dr. Langſlow, that Mr. Crowfoot's pamphlet is the joint work of me and Mr. Crowfoot. And alſo to obſerve, that I do not find in Mr. Crowfoot's book the ſentence which Dr. Langſlow has given with inverted commas, as if it had been a real quotation from Mr. Crowfoot's work.

Whether ſuch general propoſitions on the cauſe or cure of every variety of Apoplexy, can be drawn from a fair peruſal of Mr. Crowfoot's work, I ſhall leave others to determine.

But in page 561, of the 6th volume of the Medical Journal, Dr. Langſlow ſays,

" For I believe at the preſent moment there is only one opinion on the ſubject, except that of Mr. Crowfoot's, that real apoplexy is cauſed by compreſſion of the brain, and always, when the ſcull is not fractured, by effuſion of blood or ſerum.

In page 77 of the laſt Medical Journal, Dr. Langſlow ſays, " Since the publication of your laſt number, a report has been

been circulated in this neighbourhood, either from miſconception or ſome worſe motive, that in my anſwer to Mr. Crowfoot upon Apoplexy, I have intended to aſſert that no caſe of apoplexy ever *did* or *can* occur without extravaſation of blood upon the brain, &c." And in a note in the ſame page, Dr. Langſlow ſays, "Upon reference to Mr. C's ſtatement, I obſerve, that he ſays upon my return from the ſick chamber, &c. &c. to the parlour, that I declared there was *conſiderable* extravaſation of *blood* upon the brain. Now the parties who were preſent at that converſation, aſſert that they did not hear me uſe the word *conſiderable*, or *blood*." Yet in p. 561 of the Medical Journal, Dr. Langſlow gives his definition of extravaſation in the following words. "The term extravaſation ought to be uſed, when the blood or fluids eſcapes through a rupture in the coats of the veſſels."

Theſe paſſages are fair quotations from Dr. Langſlow's writings; not quotations from extracts which Dr. Langſlow might have taken from other authors; not literary forgeries. But, as I had no more ſhare in the opinions or compoſition of Mr. Crowfoot's work, than Dr. Bradley or Dr. Batty had in the opinions or compoſition of Dr. Langſlow's publications in the Medical Journal, I do not feel myſelf bound to take any farther part in this controverſy.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

I Shall think myſelf much obliged, if you will have the goodneſs to explain to your readers, why you called my card or addreſs to the Editors, *a private letter*; becauſe, a partial extract of it only being printed, and that placed in the preface to your criticiſm upon my pamphlet, it conveys an idea that it was written to deprecate your cenſure, which, in truth and juſtice both to yourſelves and my own character, I muſt wiſh to obviate.*

I feel the more anxious to remove all poſſibility of miſtake, as with inattentive or careleſs readers, it appears I have already ſuffered

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* It did not occur to us, that any one of our readers could draw ſuch an inference as this from our uſe of the words *private letter*. There is no occaſion to deprecate the cenſure of critics who give their names to the public. The only ſeverity they can exerciſe, is, to ſay nothing when they find nothing to commend. EDIT.

ſuffered from miſconception, and its effects, miſrepreſentation; not to ſpeak of the uncourteous or malevolent commentator, who may look for gratification in perplexing a queſtion which he cannot anſwer. Mr. H. Davies, in your laſt number, page 74, &c. has related two *fatal* caſes of Apoplexy ariſing from extravaſation, and then ſubjoins, "he ſhould have judged the exhibition of an *emetic* an injudicious, if not a dangerous remedy;" meaning, I preſume, that the writer of a *certain caſe of apoplexy*† would have adminiſtered ſuch a medicine.

In this remark, it is evident your correſpondent confounds the caſe of nervous apoplexy, in which emetics are frequently adviſable, with caſes ariſing from plethora, wherein probably no man would think to employ them. And as the firſt patient of Mr. Davies muſt have been in a ſtate (as often happens in ſuch fatal attacks) to render it *impoſſible* to give an emetic, I think the obſervation might very well have been ſpared.

If Mr. D. condeſcends to read my pamphlet, he will perceive, and I have no doubt candidly acknowledge, the injuſtice done to my opinions. I am, &c.

Beccles, Jan. 5, 1802. W. CROWFOOT.

To the Editors of the Medical and Phyſical Journal.

GENTLEMEN,

ON looking over your Journal for December 1801, I find a Statement of a Caſe of Apoplexy by Mr. Crowfoot, and copiouſly anſwered by Dr. Langſlow. I have taken the liberty of addreſſing to you the following Obſervations, as they occurred on the peruſal of that Anſwer; and as I conceive your ineſtimable Work is intended for the free and candid inveſtigation of whatever relates to the cauſe, treatment, or nature of diſeaſes, any apology would be unneceſſary.

To aſcertain any fact in medicine, and even ſurgery, owing to the various theories and opinions of mankind, has always been attended with much previous diſcuſſion. The ſcience is ſtill

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† As the phyſician, Dr. L———w determined the caſe to be *real apoplexy*, though *his patient* was free from every ſymptom which denotes extravaſation, exudation, or effuſion; it remained for me to prove, and I truſt it has been done ſatisfactorily, that if this patient's caſe was apoplexy, which I will not diſpute, it was unqueſtionably the nervous apoplexy of Dr. Kirkland.

ſtill imperfect, and appearances on diſſection too often only tend to prove the futility of human comprehenſion; for when there is every ſymptom of diſeaſed viſcera, how frequently does it appear to Anatomiſts, that the very part has ſuffered no derangement in its ſtructure, or has been affected in the manner the ſymptoms might naturally indicate. The moſt profound Phyſicians are never certain that their concluſions are juſt. When indulging in the enthuſiaſm of meditation, on abſorbents, excitability, or the nervous power, they are apt to forget that the human frame is the object of their inquiry; they are carried into a maze of theory and conjecture, and turn back, aſtoniſhed at the fertility of imagination and the train of reaſoning they have been led to adopt. There ſtill hangs a myſtery over many points in the animal œconomy, which puzzles the moſt acute and diſcerning to penetrate. However, there is little conſolation in being perſuaded of the incompleteneſs and uncertainty of medical knowledge; yet every ſuggeſtion or attempt for the improvement of ſcience is laudable, and cannot be unacceptable to the profeſſion. I would not wiſh to inſinuate by theſe deſultory Remarks, that what I have to advance at preſent is new or particularly important. In the Caſe of Apoplexy above alluded to, Dr. Langſlow has been very active and eloquent, to prove that the proximate cauſe of that diſeaſe is extravaſation upon the brain. I am neither acquainted with him nor Mr. Crowfoot perſonally, and I ſpeak without prejudice. He appears to me a gentleman of reſpectable acquirements, and I truſt he will hear my critique with impartiality, and without offence. Liberality of ſentiment in profeſſional inquiries certainly ought to be allowed, and every private conſideration generouſly ſacrificed to public utility.

In regard to the efforts of Nature, I do believe with Doctor Langſlow, that the *vis medicatrix naturæ* is the moſt powerful agent; that the feeble aſſiſtance of many medical practitioners can claim little praiſe; and that this agent alone, frequently expels the morbific diatheſis from the ſyſtem, in ſpite of medicine and diſeaſe. What I ſhall offer to Dr. Langſlow's conſideration is briefly this: Had there been extravaſation in Mr. Crowfoot's caſe, is it poſſible that it could be abſorbed, though after profuſe bleeding, ſo as to produce an *immediate* mitigation of every diſtreſſing ſymptom? Does not the ſucceſs of ſuch practice anſwer better to the ſuppoſition, for it can only be ſuppoſition, that there was no extravaſation, only an increaſed determination to the head, rendering the coats of the arteries turgid, and by their preſſure on the medullary portion of the brain, cauſing ſtupor, coma, or delirium? And may not the diſtended carotids, preſſing againſt the correſponding or accompanying nerves

nerves which ſupply the ſtomach, be the cauſe of the ſtomachic affection? If Apoplexy be invariably the conſequence of compreſſion of the brain, the morbid increaſed pulſation of the arteries will cauſe that compreſſion. Extravaſation, exudation, or effuſion, does not neceſſarily conſtitute compreſſion. The ſubſtance of the brain is known to be the moſt irritable part of our body, and the leaſt ſtimulus from the extreme veſſels will be the ſame, and attended by the ſame conſequences, as compreſſion; therefore, there may be compreſſion, without extravaſation, exudation, or effuſion. And although extravaſation does produce Apoplexy, may not genuine Apoplexy be produced without extravaſation? A patient with contuſio cranii will have Apoplexy when extravaſation has evidently taken place; but when another patient falls down apoplectic,* if a total abolition of the ſenſes, ſonorous breathing, the eyes vacant, and a ſpaſmodic fall of the jaw, may be allowed ſymptoms of Apoplexy; I ſay, can it be inſiſted on, that this patient had extravaſation when, *immediately* after the paroxyſm, he is as well as before the attack? Could the lymphatics of the brain, or any other power, abſorb the effuſion in ſo ſhort a time?

With reſpect to the propriety of exhibiting emetics in Apoplexy, I think the action of vomiting muſt aggravate the diſeaſe: topical applications, by diminiſhing the energy of the veſſels in the head, without debilitating the ſyſtem, muſt be eminently uſeful. In this the ideas of Dr. Langſlow are certainly correct; but when he poſitively aſſerts, "whenever Apoplexy occurs, and there is no fracture or tumour, that extravaſation, exudation, or effuſion upon the brain *always does exiſt*," I would aſk, has his impetuoſity led him to do much credit to *his* profeſſional character? for it is moſt probable that of ten caſes of Apoplexy, which are not fatal, not above one or two is cauſed by extravaſation of blood upon the brain. The following recent caſe may ſerve as an illuſtration.

Elizabeth Trinder, of Shippon, a poor girl, ſeventeen years of age, of a delicate habit, whoſe mother died of typhus † a few

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* Or as Boerhaave, nearly two hundred years ago, defined it: "Quando repente actio quinque ſenſuum externorum, tum internorum, omneſque motus voluntarii abolentur, ſuperſtite pulſu plerumque forti, et reſpiratione difficili, magna, ſtertente, una cum imagine profundi perpetuique ſomni."

Boerh. Aphor. 1008.

† Amongſt the various miſeries of which war and its concomitant evils are productive, the late ſcarcity has been the moſt ſeverely felt, and has been particularly fatal to numbers of the poorer claſſes of ſociety. Penury is the ſource of many diſeaſes, very obſtinate and alarming in their attack.

few days before, was attacked with pyrexia on the 28th of November last. She continued in a weak, debilitated state, with great depression of spirits, till early in December, when she experienced the more violent symptoms of Apoplexy. I saw her in this alarming situation; her eyes were half closed, the surface of her whole body was pale, and cold as death, and she passed her fæces and urine involuntarily. In this state she laid nearly two days insensible. Sometimes, she with difficulty lifted her hand to her head, and then dropped it as it were, lifeless on the pillow, muttering at the same time, "O my head, my head." Her respiration made a snorting noise, and she seemed hastening to dissolution. After much fruitless remonstrance, the family being persuaded that nothing could save her, they reluctantly allowed a large blister to be applied below the occiput, on the morning of the 5th of December, and in the evening two others were applied to the arms. It is remarkable, that in a few hours afterwards she raised her eyes, and spoke rationally. The pain in her head was considerably relieved. She had now some appetite, and since then her health gradually improved, and she has had few febrile paroxysms.

Dr. Langslow, I suppose, would not hesitate to pronounce, whether this was sanguineous or serous Apoplexy, and if there was in this case extravasation, exudation, or effusion. If there was an effusion, whether it was of blood or serum upon the brain, could it be so speedily absorbed? My conclusion is this, that there often occurs Apoplexy without an effusion or exudation of blood or serum upon the brain; and, that when there is *considerable* extravasations, the case is generally fatal. When there is extravasation, the patient's recovery must be slow and difficult; for the power of absorption cannot be so quick as Dr. Langslow has imagined. When the patient's recovery is *immediate*, on diminished excitement, I think it presumptive evidence, that there has been no extravasation, exudation, or effusion. The above case may stand as a confirmation

The constitution being weakened, by receiving little nourishment, is unable to bear accumulating complaints, and sinks exhausted. For when any epidemic, or other disease, is introduced into the houses of the starving poor, medicines are often of little avail. They want not only the comforts, but what is, even in health, absolutely necessary to existence. In Abingdon, the fatality of the typhus fever, amongst the poor, has been very remarkable, and generally attended with acute delirium. To the celebrated Beddoes's question, Whether children born during the scarcity, are less healthy than those born under other circumstances, or prior to that distressful period? it may be said, that his fears are unfortunately too well grounded.

firmation of the good effects of blisters, applied when there is the least hope of their being successful.

I cannot conclude this paper, without observing the dignity and importance which some attach to a physician; I mean particularly in the country. But common judgment is generally directed to the supposed rank a person holds in society, and not to his individual merits. A surgeon enjoys the respectability which follows the exercise of that department of the profession, let his abilities be great or indifferent; but to regard a man according to the endowments of his mind, seems to me most rational. It becomes no sensible man to be ambitious of a long etcetera of fellowships and titles; they will make him not one atom the wiser. It is, alas! the diploma splendors of the college, which infatuate the multitude; but when divested of these advantages, the charm is destroyed. There is a modesty in controversy which enhances merit; but Dr. Langslow assumes a confidence and a pride, when speaking of his own skill, which is little respectful. There is a delicate and a splendid way of writing on such subjects. When disputes become warm and ill-natured, they cease to be useful. Men of talents have a right to be liberal and just; but their charter does not extend so far, as to allow them to be contemptuous.

I am, &c.

Abingdon, Jan. 1, 1802. JAMES ATKINSON.

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

THE powerful effects of the Digitalis have so much engaged, of late, the attention of the medical part of the world, that we may confidently expect its real merits in the cure of diseases will ere long, by a liberal communication from those who employ it, be duly ascertained. Its power over the arterial system is so decided, as to lead to the rational conclusion, that by attentive, cautious, and judicious management, much good may be done with it; and we know, from the numerous communications which, through the medium of your excellent Journal, have been given to the world, that it may be administered with safety and effect. I have not been inattentive to its operations, and have been enabled by its use to afford very considerable relief to many patients in anasarcas and coughs, but, in no instance, with more clear and decided effect than in

 acute

acute rheumatism. It is a disease which, in the course of my practice, I have had numerous opportunities of seeing, and can bear ample testimony to the difficulty in removing it, and to the length of time necessary for that purpose. I have principally relied on large and repeated bleedings early in the disease, and have ever found them effectual, joined with an antiphlogistic regimen; but the debility which for the most part succeeds such a mode of treatment, is a matter of serious consideration; and if a medicine can be discovered which, by its effect on the arterial system, will supersede the necessity of bleeding, or even diminish in any degree that necessity, it will be a valuable discovery; for it must be allowed, that the loss of so large a quantity of blood as has frequently been found necessary to remove the disease, must have a very decided effect on the constitution, and not unfrequently lays the foundation of irremediable mischief. Having reflected much on these circumstances, and considered the wonderful power which the Digitalis possesses of diminishing the action of the heart and arteries, I was determined to employ it in the first fully formed case I should meet with. Two cases occurred nearly at the same time, one only of which I shall relate, as the treatment and event were the same in both.

On the 15th of February, 1801, I was desired to visit T. B. a robust, healthy-looking man, in the prime of life, of a florid complexion and sanguine temperament. He had been seized on the 11th with rigors, succeeded by fever, and very great pain. I found his hands, knees, feet, and shoulders much swelled, very red and inflamed, hot to the touch, and extremely painful, and he was totally incapable of moving himself. The skin felt very hot, was rather moist, and emitted that acid effluvia so peculiar to this disease, and which from long experience having found to be a constant attendant, I consider as a characteristic symptom. His tongue was slightly furred, white, and rather moist; he complained of a considerable degree of thirst, and could not get any sleep. The urine was exceedingly high coloured, and deposited a copious pink sediment, an appearance I have invariably observed in this disease. The pulse was full, hard, and beat 112 in a minute. I ordered him ten drops of the Tinct. Digitalis every six hours, and a strict antiphlogistic regimen.

My professional engagements prevented me from seeing him every day, as he lived some distance from me; it was therefore on the 17th I found that the pain, swelling, and inflammation of the joints had in no wise diminished, and the tongue and the skin were in the same state as before. Indeed, the thirst was in some degree lessened, the urine not so high coloured and turbid,

turbid, and the pulse reduced to 104. Not having had any sleep, a draught with tinct. opii, gt. xxx. was ordered at night, and the dose of tinct. digitalis increased to fifteen drops.

I was much pleased on the 19th to find the pain, inflammation, and swelling in both knees so much abated, as to enable him to move them freely. The feet and one hand were much better; the other hand and both shoulders in much the same state as before, as was also the tongue and urine; the skin moist and much cooler, and the pulse 80. The opiate had procured him a good night, which was repeated, and the tinct. digitalis continued. Not having had stools, a draught with infus. sennæ was ordered.

The pain, inflammation, and swelling, on the 21st, was confined to the right arm, all the other parts being entirely free; the heat much diminished, the skin moist, the urine the same, and the pulse 80. He complained of a slight head-ach, which however lasted but a short time. The opening draught had produced two stools, but, not having slept, the tinct. opii was increased to forty drops, the tinct. digitalis continued.

On the 23d, the redness and swelling had entirely disappeared, though a considerable degree of pain was felt in the knees and shoulders, accompanied with much weakness. The skin was cool, and both that and the tongue moist; the urine much paler, and without the copious pink-coloured sediment noticed in the beginning. The pulse 84. He had not had much sleep, and no motion; the aperient draught was therefore repeated, the opiate omitted till that had operated, and the tinct. digit. continued as before. I found he had not been so punctual in taking his medicine for the last two days as he ought. He passed the 24th and 25th much better; but having again omitted his medicine, I found on the 26th that he had a return of pain, swelling, and inflammation in the right hand and foot. The pulse was at 84, and the skin cool and moist.

By the 28th, when I again saw him, the swelling was almost entirely subsided, without the least appearance of inflammation or fixed pain; the little pain he *did* feel was slight and wandering. The skin continued cool and moist, the urine was much paler, the bowels open, the appetite returning, and he had comfortable sleep. I now ordered him mutton broth, calves-foot jelly, and sago, and reduced the tinct. digit. to three times in a day.

On visiting him March 1, I found to my surprize that the pain and swelling had returned into one hand, and, on the 3d, that it had extended to the other. The next day (4th) the symptoms were become considerably aggravated, and on inquiry discovered, that he had very imprudently, and contrary to my express directions, drank several glasses of mead. The

tongue

tongue, though moiſt, had become brown, the skin hotter, and the pulſe 92. He complained of head-ach and want of ſleep, and was ſo ſenſible of his imprudence in deviating from my directions, that he was determined to follow them implicitly in future. I increaſed the doſe of tinct. digit. to twenty drops.

The pain and ſwelling on the 7th were leſs, and every ſymptom favourable; the pulſe 96. The tinct. digit. was increaſed to twenty-five drops.

On the 10th, I was happy to find every veſtige of inflammation entirely vaniſhed; he had neither pain nor ſwelling any where; his appetite was good, the pulſe 88, and the urine now depoſited a copious lateritious ſediment. From this time, none of his complaints returned; the drops were reduced to ten, three times in a day; he ſoon recovered his former ſtate of health, and has maintained it to this day, except occaſional ſlight erratic pains, which rheumatic patients are for the moſt part ſubject to in cold weather.

I muſt here beg leave to direct your attention to the leading circumſtances of this Caſe. The ſymptoms, undoubtedly, indicated an high phlogiſtic diatheſis; and I am perfectly convinced, that had blood been drawn, its appearance would have fully confirmed my opinion: Yet was this diſeaſe, in general ſo obſtinate a one, checked in the ſhort ſpace of five days after the exhibition of the Digitalis; and for the next ſix, the progreſs towards health was conſiderable. And it cannot eſcape your obſervation, that on the medicine being omitted, the ſymptoms increaſed; but by afterwards duly perſiſting in its uſe, the diſeaſe was finally eradicated, in little more than three weeks from the time I firſt ſaw him; and this was effected without one bleeding, or the exhibition of a ſingle grain of nitre or antimony. I cannot conclude this caſe without obſerving, that it was as ſevere a one at its commencement as any I ever ſaw.

My patient informed me, that ſome years ago he was confined in the ſame complaint to his bed, *ſeventeen weeks*, during which ſpace he was bled *nine or ten times*.

It may not be improper to add, that the tincture I uſed was in the proportion of an ounce of the leaves recently dried and powdered, to five of ſpt. of wine, and was prepared under my own inſpection, with the moſt accurate attention. The leaves were gathered in the month of Auguſt, from vigorous healthy plants five or ſix feet high, growing on an elevated ſituation, and in a light ſandy ſoil.

If you ſhould conſider the publication of this Caſe in your Journal as likely to be of any advantage to the medical ſcience, it is much at your ſervice. I am, &c.

Ewell, Surrey, Jan. 8th, 1802. EDM. PITTS GAPPER.

Historical Statement of the Galvanic Discovery, and of the Publications that have appeared on that Subject.

[Continued from our laſt, pp. 70—73.]

MR. GALVANI likewiſe found, that different metals joined together in a different manner, were the moſt efficient, but above all ſilver. The experiment ſucceeded under water quite as well as in open air. It ſurpriſed Mr. Galvani, that if the hook faſtened in the ſpinal marrow was touched, the water replacing the conducting arch would excite contractions in the animal which did not enſue on the experiment being made under oil. Our author thought himſelf now entitled to aſſert, that there is in the animal a double electricity, oppoſite to one another; the one in the muſcles, the other in the nerves, or both at the ſame time in either of them. In order to aſcertain this circumſtance, he coated the nerve or the ſpine of the back with tin-foil, by which he found the motions to be extremely increaſed. The contractions however proceeded leſs vigorouſly by coating the muſcle. If a part of the nerve as well as of the muſcle was coated with a non-conducting ſubſtance, as ſilk or pitch diſſolved in oil, no contractions were excited, even on applying the conducting arch. Mr. Galvani alſo obſerved that this animal electricity, as it was ſtyled by him, opens itſelf an eaſy paſſage through ſome conducting ſolid bodies; whereas, through others it paſſes with more difficulty; but the beſt conductor of this electricity is water, while oils deſtroy the phenomena of this matter. To the conducting property of water, the author was inclined to aſcribe the fact, that having coated the ſpinal marrow, and ſeparated the lower extremities, he only obſerved contractions in the leg that was touched, which were however communicated to the other, as ſoon as he had brought both in contact with each other. Mr. Galvani proves next, in the moſt convincing manner, that the contractions are no wiſe excited by mechanical ſtimuli, as was firſt ſuppoſed by others. He alſo obſerved the contractions to ſucceed leſs vigorouſly when the muſcles were placed on a glaſs pannel, and the ſpinal marrow on an electric plate, but they became ſtronger after having changed that ſituation; moſt violently, however, they were excited, when the legs as well as the ſpinal marrow were placed on coated pannels, particularly on giving them ſome electrical ſtrokes. When the nerves were entirely ſeparated from the ſurrounding parts the contractions conſiderably increaſed. The experiments ſucceeded equally well with warm blooded and cold blooded animals. The contractions being diminiſhed,

minished, were restored to their former vigor after a time of rest had been allowed.

We must acquiesce in mentioning only the conjectures and conclusions which the author has drawn from his discovery, without farther entering into the subtle arguments he has brought forth in support of his ideas. The conclusions he builds on the facts related by him, are as follow: Animals are endued with a peculiar electricity, to which he gives the appellation of *animal electricity*, and which he thinks to be contained in most animal parts, chiefly however manifesting itself in the muscles and nerves. It seems to be secerned in the brain from the blood, whence it is communicated through the nerves to the different parts of the body, but it appears to reside chiefly in the muscles. A muscular fibre is similar to a Leyden phial, and the nerve represents the conductor of the phial, and consequently the whole muscular substance is to be considered as a number of Leyden phials. The external surface of the muscle possesses negative, the internal substance positive electricity. The interior of the nerves is composed of a matter capable of conducting electricity, while the exterior prevents by the oily coating its effusion and dispersion. Muscular motions proceed, when the electric fluid is conducted from the interior of the muscle into the nerve, whence it is brought back again to the muscles, either through the external fluid of the nerves, or through the membranes and the adjacent parts, as it were through an arch, so that, according to the laws of equilibrium, the same quantity may be united in the negative electric part of the muscular fibre, which issued by means of the stimulus in the nerves from the positive electrical part. Dr. Eusebius Valli relates, in his Memoir on Animal Electricity, a series of thirty-two experiments, by which he particularly endeavoured to determine the effects of tobacco and opium on Galvanism, but he found these substances, as well as other poisons, to have no action on the electrical principle; the gases, however, do violently affect it.

In a letter from Mr. Bossano Carminati to Mr. Galvani, the following notice is given of the results of Mr. Volta's experiments. "Prepared frogs, whose spinal marrow and part of the nerves are coated with metallic plates, may be used as the most sensible electrometers. The negative, and not the positive electricity, is seated in the nerves." Mr. Volta describes the experiment from which he has drawn that conclusion, in a letter to Dr. Boronio. "Not being able to trace the nature of this very weak electricity by means of the most sensible electrometer, I proceeded in another manner. I called to my mind, that two phials being brought in contact with each other by their

their ſynonymous electrical ſurfaces, do not diſcharge themſelves, which however is the caſe, as ſoon as they touch each other with their oppoſite electrical points; and I thought it therefore not indifferent, whether I applied the internal coating of a weakly charged phial to the muſcle or to the nerve. On making a ſeries of experiments according to this idea, I have frequently obſerved, that on applying the poſitive ſurface of a phial to the nerve, $\frac{5}{100}$ or $\frac{6}{100}$° of the electrometer, did ſuffice for producing contractions in a frog, whereas hardly from $\frac{20}{100}$ to $\frac{30}{100}$° were ſufficient for the ſame purpoſe, when the poſitive ſurface of the Leyden phial was brought in contact with the muſcle, and the negative ſurface with the nerve. Thence we may conclude, that a negative electricity is imparted by Nature to the nerve, and a poſitive electricity to the muſcles." Mr. Galvani, in reply to this, writes in a letter, addreſſed to Mr. Carminati, as follows: "Could we not think it probable, that in the caſe where the head of the Leyden phial was brought in combination with the nerve, the convulſions might have entirely or for the moſt part been owing to the electrical matter having penetrated from the internal coating of the phial into the nerve, and thence into the internal ſurface of the muſcular fibre? For though, according to my hypotheſis, a plus of electricity exiſts in the head of the phial as well as in the interior of the muſcular ſubſtance, both electricities are moſt probably efficient in the ſame degree of force; and it is moſt likely that the weak natural electricity of the nerves is overcome by the ſtronger artificial electricity. We may therefore aſſert, that a part of the poſitive electricity of the phial penetrates into the internal ſubſtance of the muſcles by means of the conducting ſubſtance of the nerves, where, being aſſimilated to their natural electricity, it occaſions a diſcharge, which is not produced by a proceſs of the animal machine, but effected by the Leyden phial in the hands of the experimenter."

[To be continued.]

On Medical Degrees.

[Continued from our laſt, p. 13.]

IF no radical cure can be obtained for this abuſe, might it not be proper to add a diſtinguiſhing mark to ſuch Phyſicians? Let them be pointed out as *impoſtors* of a *double dye*. An impoſtor who claims a title at his own hand may, perhaps, be excuſed; but the men who *falsley* obtain, and thoſe who *falsley* confer the

the sanctions of a University, are certainly the most abandoned. The degree of a University should be sacred to merit. When a man assumes the title of Physician, the public naturally confide in his knowledge and abilities: but when to this, he publishes the sanctions of a University, their faith becomes stronger. What must be the result of this confidence in the case of a man destitute of knowledge? It is like committing the charge of a ship, in the most critical situation, to a man who has never handled a rudder, nor seen a compass.

When Dr. Johnson was at St. A******, in his tour to the North of Scotland, he was frequently speaking of the original splendour of the place. The Professors and other people about the University assured him, that the present decline was not owing to any deficiency on their part, *but they were very poor.* "Never mind that," says the Doctor, "you will get rich by DEGREES." This is now literally the case; for since the scheme of publishing a list of all the physicians took place, the applications have been very frequent, and the conferers of these honours are actually flattering themselves that they shall get "Rich by Degrees." Men, who could not even be admitted to public examinations, as surgeons, are now dashing away with their degrees as physicians! If this was a matter of notoriety at the time Johnson made his tour, it is still more so now.

By these reflections I am certain to gain their ill will. But as I have now set out in the cause of the public, I am determined to go on; and if any of *these Physicians* consider themselves as injured by the likeness I have drawn of them, I shall be happy to hear their defence, and give them every satisfaction in my power. You may expect to hear further from me on this subject. I am, &c.

Scotland, Dec. 1, 1801. PHILO MEDICUS.

P. S. In my next, I shall present you with a few portraits of our St. A****** Physicians.

CRITICAL

CRITICAL RETROSPECT

OF

MEDICAL AND PHYSICAL LITERATURE.

[FOREIGN AND DOMESTIC.]

Obſervations on the Cancerous Breaſt, by Dr. Adams.

[Continued from our laſt, pp. 82—89.]

The peculiar nature of the *fat* of a carcinomatous breaſt is likewiſe very forcibly contraſted with the healthy ſecretion.

" You admit the propriety of my remark, that the fungus divides the fat into diſtinct portions; but you object to my addition, that the fat thus encloſed ſhould be free from *common* cellular ſubſtance. Thus then we agree that there is a fungus, or if you prefer the term, a ſcirrhus, which divides different portions of fat from each other.

" Before I explain the peculiarity of this encloſed fat, let me in my turn beg you to account for the great quantity of fat you find in a truly carcinomatous breaſt. Compare the ſize of the two breaſts in the ſame ſubject, and examine how much of the increaſed ſize in the diſeaſed one is made up of fat. Is it probable that ſuch an increaſed quantity ſhould be a healthy ſecretion, or that it ſhould even be depoſited by any proceſs ſimilar to what takes place in a ſtate of health? I admit with you that it has the ſame appearance, and alſo that it has a cellular apparatus. But this appearance is only the ſame when the amputated part has been kept long enough for the fat to conſolidate, and the cellular apparatus is altogether different. Inſtead of the apparently irregular net-work, which forms the common adipoſe cellular membrane, and which is ſo entirely intermixed with fat, that without cutting into ſmall pieces and diſſolving by heat, it is impoſſible to ſeparate fat from membrane, the cells of true carcinoma are ſo many ſmall and firm capſules, each containing its diſtinct portions of fat, without connection of each other but by lateral adheſion."

Having raiſed his theory of the hydatid-nature of carcinoma on a foundation which many of our readers will probably conſider of very doubful ſolidity, Dr. Adams proceeds to a ſuperſtructure ſtill more bold and original, which is the animalcular nature of *ſteatoma*. The author indeed is aware of the objection which inſtantly ſuggeſts itſelf in the total want of *motion* or *muſcular contraction* in ſteatomatous tumours; which property he had endeavoured to exhibit in the papillary appearance aſſumed by the cells of carcinoma when cut tranſverſely, after all connection with the body had been removed

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by amputation. He obferves with perfect propriety, that as mufcular contraction is confidered by Dr. Baillie as one of the properties of life, and the ftrongeft proof of its prefence, and as carcinoma poffeffes this property but fteatoma does not, the proofs of the vitality of the former are independent of any analogy with the latter; and therefore, as they are not fupported by fuch analogy in fome particulars, they do not fail from the want of it in others.

This obfervation however only applies to the queftion of the vitality of carcinoma, and the objection which will be directly urged againft the feparate exiftence of fteatoma, from its want of mufcular contraction, remains in its full force. This objection our readers will perhaps think, is hardly obviated by the following remark: "Though no contraction can be perceived in the tunics of "moft other encyfted tumours," (than carcinoma) "yet this may "arife from the contents of fome being too folid, and others too "fluid, to form the papillary appearance I have defcribed in car- "cinoma." In a note, however, the author obferves, that in the watery encyfted tumour (or the lymphatic hydatid in a folid part as he would term it) on being punctured before its death, the contained fluid, though turbid, efcapes with confiderable force like water from a compreffed bladder; which he is inclined to impute to mufcular contraction.

The arguments for the feparate vitality of fteatoma and tumours of a fimilar nature, the author gives in the following terms, "Thus, without further preface, I am free to acknowledge that not only fteatoma, but atheroma and meliceris, as they have been called fince the days of the Greek phyficians, that is, all encyfted tumours, whofe cyft and contents have no communicating branches with the furrounding blood veffels, appear to me animalcular, or at leaft to have the fame œconomy as has been admitted in hydatis lymphatica.

That this is the cafe I conceive:

Firft, Becaufe they are all found in the fame parts of the body, and often in the fame individual tumour.

"Secondly, Becaufe they are all free from any communicating branches in the furrounding blood-veffels.

"Thirdly, Becaufe they all appear to have a power of growth, after which they die, without otherwife affecting the body in which they exifted, but by their local ftimulus.

Fourthly, Becaufe the cyft containing either of them is incapable of fuppuration, and fubject to none of thofe laws, by which capfules formed to prevent the diffufion of matter in abfcefs, or fuppuration, or original tunics when preternaturally diftended with fluid, are governed.

"Fifthly, Becaufe a fimilar mode of multiplication may be traced in each."

The firft pofition he illuftrates by the examples of the breaft, uterus, and tefticle; and the abdomen, in which fteatoma is often found

found in the ſame cyſt with the lymphatic hydatid, from the authority of Haller and others.

The ſecond argument is univerſally admitted without diſpute. The author likewiſe notices the difference betwen the cyſt of a watery encyſted tumour and the capſule of a common abſceſs. The latter, Mr. Hunter has ſhewn to be formed by an effuſion of coagulable lymph which confines the pus within the cyſt, and alſo plugs up the extremities of any contiguous blood veſſels, ſo as to prevent hæmorrhage. On the other hand, the tunic of encyſted tumours is compoſed of regular concentric ſtrata (of which Dr. A. has ſeparated not leſs than ten) and the blood veſſels are not obliterated ſuddenly by coagulated lymph, but ramify with extreme minuteneſs over the coats of the cyſt, but never in any degree enter its contents, whether ſolid or fluid.

The third poſition, Dr. A. attempts to eſtabliſh by ſuppoſing that the uncertain progreſs and often ſtationary condition of ſteatoma may be ſuppoſed to be the *torpidity* of the hydatid, ſimilar to the ſtate of the egg before incubation; and that the ſuppuration of meliceris and the gangrene in ſteatoma are the death of the hydatid, which then acts as an extraneous ſubſtance within the living body, and thus becomes diſcharged.

The neceſſity of diſſecting away every part of the ſac of encyſted tumours in the operation for their removal, and the impoſſibility of making it the baſis for granulation and ſuppuration, prove a very marked difference between theſe tumours and common abceſs, and ſufficiently illuſtrate the author's fourth argument.

The fifth poſition Dr. A. endeavours to prove by the known progreſs of ſeveral caſes of ſteatoma, in which the diſeaſe increaſed by cyſts or in cluſters. The author concludes this laboured and ingenious illuſtration by pointing out the peculiar points of difference between ſteatoma in its various forms and carcinoma; the principal of which is, the property which the latter poſſeſſes of producing fungus, as has been already mentioned.

The concluding letter in this volume is addreſſed to Dr. Stokes, and contains, along with a recapitulation of the author's peculiar opinions, ſeveral practical remarks on the treatment of cancer. Dr. A. denies that the corrugated redneſs (from which the term *cancer* was ſuppoſed to derive its origin) ariſes from varicous veins, but thinks that the appearance is to be attributed only to the thinneſs of the integuments previous to ulceration. The author's opinion on the origin of the term cancer is at leaſt as plauſible as the other. It is as follows:

"As to the term cancer, it appears to me that the ancients had a much better reaſon for the uſe of it; I mean its property of proceeding *backwards*, or contrary to the progreſs of abſceſſes, which is towards the ſurface. Whilſt the fungus is extending to the ſkin, the carcinomatous hydatids are multiplying internally, and ſeem to avoid the ſkin as much as poſſible. When ulceration has begun, it is not, as in common abſceſs, becauſe matter has approached the ſurface with a previous elongation and, as we call it, pointing of the ſkin;

skin; for the fluid, which consists only of hydatids altered in their form by the loss of life, makes no progress towards the surface, but remains till the gradual sloughing and ulceration of the fungus expose the cavity in which they are contained. When by these means the fragments of the cysts and turbid fluid into which the hydatids are converted by death escape, the sides of the cavity do not collapse like that of common abscess, but expose a ghastly cavern, from which, instead of pus, a watery discharge is secreted, attended with a very peculiar smell. Round the mouth of this cavern, fungus sometimes grows; and instead of any attempt to unite, the skin curls in a contrary direction, which increases the depth of the cavity. This continues as long as any fragments of hydatid-tunics retain their life. As they die, ulceration takes place to detach them, like other extraneous bodies; and this ulceration being unattended with fungus, renders the cavity for a time still deeper. After all the fragments of tunics are detached, the wound will perhaps continue stationary or heal, or more commonly another set of hydatids will be so far advanced as to produce a fresh sloughing or ulceration.

"If this progress of the disease from the surface inwards was sufficient to admit the allusion to the supposed motion of a crab, the more rapid cases might with still more justice; for in these the patient will sometimes be destroyed before the skin is broken."

Dr. A. agrees with his correspondent, that in a warm climate this dreadful malady is less frequent and less severe than in cold.

The succeeding observations relate to the important questions, Whether the operation should be performed at all? and, Whether any other mode of treatment will produce a cure? The author adduces the opposite testimony of two highly respectable authorities, the late Dr. Alex. Monro, and Mr. Hill. The former observes, that of sixty cancers, at the extirpation of which he had been present, only four patients remained free from the disease for three years (a very alarming prognostic for the operator!) Mr. Hill on the other hand says, that he has extirpated no less than eighty-eight genuine cancers, eighty-four of which were ulcerated, and all excepting two recovered of the operation. Mr. Hill's cases however were probably, he thinks, chiefly of the lip, which are less formidable than in most other parts. Dr. A. also notices the valuable statement given by Mr. Fearon; and he adds the following remark, in which we believe he will be very feelingly joined by a great number of practitioners.

"If I were to give the result of my own observation, I know not how to account for it, but where every thing has appeared to succeed, the patients have in many instances died within a year or two after the operation. Where they have lived longer, the disease has sometimes re-appeared in a few months; sometimes at a period so remote, that all apprehensions concerning it were at an end. It is however much more satisfactory to give the result of other mens' practice than our own, especially when their cases are related without any bias towards our own opinion."

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The following injunctions for the performance of the operation merit attention.

"The first step appears to me too often neglected, which should be to compare the diseased with the sound breast; and if we find a greater fulness in the former towards the neck and axilla, whether attended with stony hardness or not, we may depend on it that the disease extends beyond the indurated part. If a hardness in the axilla is attended with this fulness in the parts above-mentioned, we have every reason to believe that the multiplication of hydatids extends regularly from one perceptible induration to the other, though no fungus can be felt except in those parts where the hydatids, from the greater rapidity of their growth and multiplication, or from their approaching nearer the skin, have stimulated the parts to the production of this fungus. In this case, to remove all the disease would be to remove the whole breast and neighbouring cellular substance, extending superficially from the sternum to the axilla, and in depth to the pectoral muscle under the clavicle and still more inaccessible parts.

"If at the same time the skin should be so much diseased, that it would not be easy to leave enough to cover such a surface, nothing could excuse such an operation but the importunity of the patient, or a confidence in the operator, that he has courage and perseverance equal to such an undertaking.

If the lump seems circumscribed and the axilla clear, the next enquiry is, whether every other part of the breast appears similar to the sound one. In this examination some latitude must be admitted, the left breast being usually somewhat larger than the right. But this must be referred to the recollection of the patient at a period before the disease could have been suspected. We must also make allowance for any occasional exacerbations of inflammation, which extending much beyond the tumour by producing an increase in the size and number of vessels, will for a time enlarge the whole breast."

After the mass of the carcinomatous tumour is dissected out, it is usual for the operator to determine whether the whole disease is removed, by feeling if any hardened lumps still remain.

To this Dr. A. observes, that "Nothing can be more fallacious than such an attempt. The surface is often so much covered with blood as to render an accurate inspection impracticable, and carcinomatous hydatids may exist without a fungus, and consequently without a perceptible induration. Our attention therefore should be directed to the cut surface of the amputated part. This we can examine in the most favourable points of view without incommoding our patient, or without being interrupted by any bleeding vessels. If we find on its surface either an appearance of fungus or of that greenish yellow fat which I call the carcinomatus hydatids, or any thing different from the natural healthy state, we must apply to the corresponding surface of what is left, and renew our operation till what we cut off has a natural healthy appearance."

Concerning the use of the arsenical caustic, the author is decidedly

edly of opinion that the few cases in which this practice has succeeded, have been those of scrophulous or steatomatous tumours, in which the whole sac with its contents has sloughed out, and left a clean granulating surface to fill the cavity thus left. He observes however, and we think very justly, that true carcinoma will be dreadfully exasperated by the caustic mode of extirpation; and that the want of integuments whereby to cover the wound and heal it by the first intention, is a most serious objection in a wound so disposed to the formation of fungus.

A variety of observations on the use of internal remedies conclude this letter, and with it the volume.

We have thus given our readers a very detailed account of this ingenious and very original treatise. We cannot entirely say that the author has made out, in a satisfactory manner, the justness of his singular hypothesis; but the work shews so much acute reasoning and accurate observation, that it will ensure to its readers entertainment and profit, if it should not enforce conviction.

A Treatise on the Cow-pox; containing the History of Vaccine Innoculation, and an account of the various publications which have appeared on that subject, in Great Britain, and other parts of the world. By JOHN RING, Member of the Royal College of Surgeons in London. Part I. 8vo. pp. 496. London, 1801. Carpenters.

THE author explains his reasons for publishing this elaborate work in *parts* in his Preface. "The vast number of remarks published on Vaccine Innoculation, both at home and abroad, having swollen this Treatise far beyond the bulk at first intended, it was deemed expedient to divide it into two parts. The continual occurrence of new facts, and publication of new treatises on the subject, rendered it impracticable to preserve a strict and methodical arrangement. The intention of the author was, to collect and combine the substance of all that has hitherto been ascertained on this interesting subject; and rather to incur the censure of prolixity, than to deserve the charge of omitting any thing of importance, on an occasion where the welfare and happiness of the whole human race are so immediately concerned.

"In some measure to supply the want of systematic order, a copious Index will be subjoined to the second part. Two plates will accompany that part, which will unavoidably cause an addition to the price. The difficulty of procuring accurate representations of the Vaccine Pustules has delayed the publication of this work; and, it is hoped, the reason, when understood, will plead a sufficient apology for that delay. It was the author's wish, to have given one plate with each volume; but he was unwilling longer to defer, what, he sincerely hopes, may prove useful."

Mr. Ring then, with his well known ability for the task, pursues the history of this very interesting subject from the first mention of it in London down to the present time. He does not, however, think it necessary to follow chronological order on all occasions; but,

but on the contrary, he brings all the material evidence which may tend to illuſtrate any important ſubject, as far as poſſible, into one point of view, though developed at different times. For inſtance, the information reſpecting the origin of the virus, received only a few months ago, will be found under that head, at the beginning of the preſent volume, to page 32. There can be no doubt that every Practitioner, who thinks it his duty to acquire the moſt ample and correct information on Vaccination, will peruſe Mr. Ring's elegant and comprehenſive work. Our readers will not expect that we ſhould give extracts from Mr. R's Treatiſe, becauſe we have devoted ſo large a portion of the Journal to the elucidation and confirmation of this ſubject from its commencement. The zeal and perſeverance with which Mr. R. has traced and expoſed the falſhoods and miſrepreſentations, fabricated by ignorant, envious, and malignant practitioners, merit the applauſe of his cotemporaries, and will ſecure that of poſterity.

Oſſervazioni Pratiche sull'uso del Vajuolo Vaccino, come Preservativo del Vajuolo Umano; i. e. *Practical Observations on the Use of the Cow-pox, as a Preservative against the Small-pox.* By LEWIS SACCO, M. D. 8vo. pp. 216. Milan.

(With Plates.)

SOME extracts from this publication cannot fail of being intereſting to the public, as they ſhew in what light the diſcovery of the Cow-pox is conſidered among foreign nations. The author is a medical man of great eminence in Italy, and it is probable, from his great ſucceſs, that his ſentiments will be ſpeedily adopted over all that country. In his Introduction, he thus ſpeaks of the Cow-pox, and the great advantages reſulting to mankind from its diſcovery.

"Among the very great number of experiments which our age boaſts, one of the moſt important, and one which will make an epoch in the annals of medicine and of humanity, is the Inoculation of the Cow-pox, by means of which man is preſerved from the infection of the natural Small-pox, and its dreadful conſequences. Still indeed we are unable to trace the connection that exiſts between the pus of the ordinary Small-pox, and that which is extracted from certain puſtules which appear under the udders of cows, and which on that account are called the Cow-pox; but it has been happily demonſtrated, that the latter being introduced into the human ſyſtem, after a ſhort period of ſymptoms by no means ſevere, becomes a complete preſervative againſt the Small-pox, which, though ſometimes a ſlight diſeaſe, frequently deforms the perſon, inflicts irreparable wounds on the moſt tender parts of the frame, and often ends in its deſtruction. The diſcovery of inoculation for the Small-pox furniſhed the means of preventing, in a great meaſure, theſe diſaſters; but this practice was very ſlowly adopted, and we in Italy particularly ſuffer ourſelves ſtill to be expoſed to the ravages of the natural Small-pox. It would be a long detail, were I to recount all the teſtimonies of celebrated writers concerning the horrible ravages of this diſeaſe. According to the moſt moderate calculation, ten out of every hundred attacked by

the natural Small-pox, fall victims to it; and if we estimate by this computation the number who are cut off by its ravages, during the course of twenty-five years, Europe in that period suffers from this cause the loss of fifteen millions of its inhabitants. In America, where the natural Small-pox is of a still more fatal nature, it carries off twenty, thirty, and even forty in every hundred."

After having produced many other striking facts, which are already well-known in this country, to shew the disastrous effects of the natural Small-pox, and the benefits arising from inoculation; the author proceeds to the discovery of Dr. Jenner, justly extols it as a benefit conferred on the human race, and proves, from the united results of numberless experiments, that it completely prevents any attack of the natural Small-pox, and is infinitely more safe and easy in its operation than the mildest inoculated Small-pox.

He then pays the following just tribute to the unwearied industry and perseverance of Dr. Jenner, lamenting at the same time the slow and "unwilling gratitude of base mankind."

"But this discovery, so fortunate for the human race, shared the fate of other grand and useful innovations, by encountering much opposition at its first outset. The basest envy let loose all its virulence against the discoverer on his appearance in London; but its attacks only made him redouble his diligence to bring his discovery to perfection. For a moment he retired from his enemies, to confound them on his return with the victorious arms of multiplied observations, and the most decisive experiments. At a distance from the clamours of a populous city, in the retirements of Gloucester, where the Cow-pox is almost endemic, Jenner had an opportunity of continuing his observations in the fullest tranquillity; and after a very great number of experiments, he resolved no longer to doubt of the immense advantages to be derived from the results he had obtained. At the same time he wrote down the whole series of his observations; and placed his ideas in such a clear and strong light, that he excited in every one a desire to examine their justness, by following the steps of the author in his investigations. The most eminent medical men in England began to turn their attention towards the Cow-pox. And as it is the fate of all the most useful discoveries, to meet with opposition and obstacles, which, by exciting the general curiosity, fix the public attention upon them, Jenner reaped this advantage from the virulence of his enemies, and after many struggles between prejudice and good sense, between various passions and the direct guidance of reason, truth finally triumphed, and the most celebrated medical men of England, convinced by their own experience and the observations of others, declared themselves advocates for the Cow-pox.

"The inoculation of the Cow-pox was carried over in a short time to our continent, and was most readily adopted in France, where that enlightened government, with the assistance of the National Institute, made the experiments be repeated, and afterwards multiplied

multiplied with a like happy ſucceſs through the departments of the Republic. At Ginevre particularly, the chief town of the department of Lemagne, many learned and enlightened men ſet the example of following with a ſolicitous attention the ſteps of the Engliſh diſcoverer. Here a great variety of experiments were made on the Cow-pox, in which the illuſtrious profeſſor and phyſician Odier, as well as Tourlet and Pictet particularly diſtinguiſhed themſelves; and publiſhed on that ſubject moſt inſtructive memoirs, which have ſerved as a guide to medical men in many other countries, and prompted them to endeavour likewiſe to deſerve well of their country and of the human race. About the ſame time, the Cow-pox was introduced into Germany, and with ſuch ſucceſs as to be generally ſubſtituted to inoculation for the Small-pox, as it had been done in England and France. Notwithſtanding the evidence of theſe facts, and of this common conſent of the greateſt nations in adopting the Cow-pox, there were not wanting, both in France and Germany, opponents of the ſame claſs with thoſe that Jenner had to encounter in England. Some of them, before knowing the reſults of experiment, began to oppoſe it, and to decry the inoculation of the Cow-pox as a dangerous practice, that introduced into the human ſyſtem a morbid matter taken from brutes. But this ſort of oppoſition was ſoon forced to give way to the immenſe number of experiments, not one of which was clouded by an unfavourable iſſue; and the enemies of the Cow-pox were in a ſhort time reduced to a ſilence fortunate for humanity.

" In Spain alſo, which vies with other cultivated nations in uſeful ſtudies, and in application to thoſe things that regard the proſperity of ſociety, the Cow-pox began to be inoculated; and the firſt attempts having ſatisfied the moſt ſanguine hopes of ſucceſs, will no doubt ſoon render the practice general in that vaſt kingdom and its populous colonies. I have been certified of theſe particulars by Seignor de Condado, his Catholic Majeſty's Chargé d'Affaires with the Ciſalpine Republic, a man equally reſpectable for his learning, his philanthropy, and the diſtinguiſhed rank he holds as the repreſentative of his court. He likewiſe communicated to me the following article, which appeared in the Madrid Gazette, January 6, 1801, and which is to be conſidered as authentic. " Dr. Don Franceſco Piguillem, a phyſician of Puigerda, wiſhing to find the truth of thoſe prodigies attributed to the Cow-pox by the moſt celebrated French and Engliſh phyſicians, cauſed a portion of the virus or vaccine matter to be ſent him from Paris; and with it he inoculated four boys, the 3d of laſt December. Although the operation was performed in the coldeſt and moſt ſevere days of this winter, the eruption took place, and followed its courſe with the greateſt regularity, without the boys loſing their chearfulneſs even a ſingle day, or ſuffering the ſmalleſt uneaſineſs. On the 15th of December, which was the 12th of the inoculation, the ſame profeſſor vaccinated ſome other boys with matter taken off the firſt; and this ſecond operation was performed in the preſence of the governor of the city and other perſons of

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distinction. The benign nature of the Cow-pox, the regular course it observes, and the result, which was entirely conformable to the descriptions of the English and French physicians, makes Dr. Piguillem hope that this invention will in a few years entirely banish the Small-pox."

"But since the inoculation of the Small-pox has not met among us with the reception it deserved, and since by this means the course is left free to the destructive influence of the natural variola, every good Italian ought to hope, that in the present happy epoch, he will see adopted the Vaccine Inoculation, which possesses so many and so remarkable advantages over the former inoculation. It is designed as the safe-guard of the human race against the natural Small-pox; and I have voluntarily undertaken the present work with a view to inform our governors and every other class of men, of the experiments which others have made with the most happy result, as well as those which I have myself conducted in considerable numbers, and with equal success."

It may not be unacceptable to the English reader to have some extracts from Dr. Sacco's description of the Cow-pox, as his ideas differ in some particulars from those received in England, and as he asserts that the matter of the Cow-pox is to be found indigenous in Lombardy.

"The Cow-pox is a malady which, till the present time, entirely belonged to the veterinary department; and was so confounded with other cutaneous eruptions, that no one thought it worth particular attention. Nor is it to be wondered that this eruption was treated with such negligence, for its mild nature and short duration rendered its inconveniencies extremely trifling: Nay, there are some countries, where many assert, probably through want of attentive observation, that it does not exist in any shape whatever; and these people will have it, but without any foundation to their opinion, that this disease is exclusively confined to the west of England. From hence it proceeds, that scarcely a single veterinary writer gives a proper description and diagnosis of it. There is another disease found among cattle, which resembles the Cow-pox, but ought by no means to be confounded with it; as the former is a most contagious disease, and though not in itself of a mortal nature, it reduces the cattle to excessive leanness, and is in danger of destroying them if it be neglected at the commencement. The Cow-pox, on the other hand, is a disease equally contagious indeed, but so mild as scarcely to render the cow sickly, yielding in a few days without any assistance, and effecting of itself a complete cure.

"Upon examining the diseases which attack the cows of Lombardy, it appeared beyond a doubt that the Cow-pox is not peculiar to England; for upon examining our farmers, and those who are usually employed in the care of cows, we find that the herds, the cows of which are mostly of the Swiss breed, at certain times, particularly in the spring and autumn, are attacked with certain pustules on their udders. These pustules, after passing through the ordinary

ordinary ſtages in the ſame manner as the Small-pox, terminate in encruſtations, which ſometimes fall off in a ſhort ſpace, and at other times remain many days, occaſioning a painful ſenſation to the cows when they are milked, and thus incommoding thoſe who are employed in that buſineſs. The ſymptoms which uſually accompany this diſtemper are, diminution of appetite, averſion to food, continual rumination without any materials in the mouth; and, to uſe the expreſſion of the herdſmen who have obſerved it, the cows make a motion with their lips, reſembling that which men make when they play on the pipe. The milk then becomes leſs in quantity, and much thinner; the eye ſomewhat downcaſt. In the progreſs of the diſtemper there appears a ſlight degree of fever, which is followed in two or three days by an eruption of ſome puſtules on the lower part of the udder, and particularly on the nipples. Sometimes, but rarely, a few appear about the eyelids and noſtrils; theſe riſe into little ſwellings with their centre more depreſſed, of a ſhining appearance and a reddiſh brown colour, and contain a thin inodorous fluid, which thickens, and remains in the puſtules. Theſe, after two or three days, ſuppurate, change into very thick encruſtations of a deep red, and render it painful and diſagreeable to the cows to be milked. Theſe encruſtations, before arriving at their complete exſiccation and detachment, paſs through a period of eight or ten days. This ſpecies of diſtemper, which is not very frequently obſerved, is contagious to ſuch a degree, that if one cow contracts it, in the courſe of ten days the whole herd is infected. There is another ſpecies of diſtemper exceedingly like this in the mode of infection and the progreſs of ſeveral of its ſymptoms, but they differ in ſeveral particulars, the latter being accompanied with no fever, and being much ſhorter in its duration, terminating in eight or ten days, during which period the encruſtations, which are much ſmaller than in the former diſtemper, fall away.

"Such are the ſymptoms obſerved in Lombardy during the progreſs of Vaccine diſtempers; and it is beyond doubt, that the Cow Pox found among the cows of Lombardy, is of a much milder nature than that known in England. This is evident from the deſcription which I have given, and that given by Jenner of what he found in his own country.—I now proceed to give an account of the ſymptoms which I have regularly obſerved in thoſe I have inoculated with the Cow-pox.

"Upon inoculating a man with the matter found in the puſtules of the real Cow-pox above deſcribed, ſeveral of its ſymptoms reſemble thoſe obſerved in the Small-pox; they are, however, diſtinguiſhed by one eſſential difference, which is, that the Cow-pox produces no general eruption, but remains limited to the arm, or to the very ſpot where the matter was introduced. This difference alone is ſufficient reaſon for always preferring the Vaccine Inoculation; ſince the Small-pox frequently produces an abundant eruption over every part of the body, which frequently becomes confluent, not only deforming the countenance, but even endangering the life. For the moſt part, thoſe inoculated with the Cow-pox are

are attacked about the ſixth or ſeventh day with a fever, uſually ſlight, but ſometimes more ſevere, and attended with cold and hot exacerbations, which laſt one, two, or three days. This fever is accompanied with nauſea, ſometimes though rarely with vomiting, and ſome convulſive movements of ſmall moment. The ſmall inciſions or punctures made by the inoculation, at firſt aſſume the appearance of a fly-bite, and at the ſame time there is produced a ſlight degree of hardneſs in the cellular membrane, which is a certain ſign that the inoculation is to take effect. Theſe punctures are afterwards ſurrounded with a red eryſipelous zone; they then ſwell and riſe into large puſtules of a clear pale red, which retain a limpid humour till ſuch time as they open of themſelves; at that period, which is uſually towards the ſeventh or eighth day, there proceeds from them a humour ſlightly viſcous, of a colour ſcarcely tinctured with a very pale red, and by no means odorous; after ſuppurating for two or three days, they become dry, and are converted into thick encruſtations, which after eight or ten days more uſually fall off. It ſometimes happens, particularly when the inciſions are quickly converted into puſtules and become ſoon exſiccated, that a new inflammation ſucceeds to the cruſt already formed; the fever, which frequently is not perceptible during the firſt period, now makes its appearance; ſome pains are felt under the arm-pit, the glands ſwell, the encruſtations are ſurrounded with a redneſs brighter and more extended, new puſtules of a large ſize appear around the firſt. Theſe puſtules, containing the fluid above deſcribed, after a new inflammation of three days, open of themſelves, ſuppurate, and uſually become dry; ſometimes but very rarely, the puſtules are converted into little ulcers, which being ſuperficial, are attended with no great inconvenience. Theſe laſt very ſingular phenomena ought never to be loſt ſight of by inoculators, that they may not run the riſque of looking upon the courſe of the Cow-pox as completed, or apprehend that the inoculation has miſcarried, when ſome of its ſymptoms may revive and crown the operation with the deſired ſucceſs."

The author afterwards quotes Dr. Jenner's deſcription of the Cow-pox, and concludes that the ſpecies found in Lombardy is of a milder nature than the Engliſh. In treating of the origin of the Cow-pox, he does not admit Dr. Jenner's opinion about its being ultimately derived from the greaſe of horſes. His obſervations on this ſubject may be intereſting to our readers.

" If we carried our account of the Cow-pox back to the origin of contagious diſorders and their true cauſes, we ſhould involve ourſelves in a maze of obſcurity, and contribute nothing to the advancement of ſcience. According to the aſſertion of Jenner, the origin of the Cow-pox is to be deduced from another diſtemper not proper to this animal. He alledges, that the exciting cauſe of this infection aroſe from another diſeaſe to which horſes are ſubject, called among the Engliſh the greaſe, and that thoſe people who were employed in the cure of ſuch ulcers, going afterwards to milk the cows with this matter on their hands, communicated

municated to them a new ſort of diſtemper. Theſe aſſertions are nothing more than ſimple conjectures; but as they come from Jenner, ſupported by various facts, they may on this account ſeem worthy of notice and diſcuſſion. Nay, more, the Engliſh writer will have it, that not only the matter which iſſues from the *grease,* but matter from any other equinous ulcer whatſoever is able to produce in cows the ſame infection. I ſubjoin ſome obſervations on which Jenner has founded his theory and reſts his opinions." After quoting ſome facts adduced by Dr. Jenner in ſupport of his theory, Dr. Sacco proceeds: "From theſe facts Jenner concludes, that the introduction of the *greaſe* into the human ſyſtem, may, perhaps, preſerve it from the attacks of the natural Small-pox; but as in many caſes the reſult of his obſervations is not conſtantly the ſame, the Small-pox ſometimes producing an equivocal eruption, he thinks it neceſſary that the pus of the greaſe, or any other equine ulcer ſhould come newly modified through the medium of cows. This much is certain, that if the origin of the Cow-pox can and ought to be attributed to the greaſe, as the genius of the diſtemper is different in horſes and cows, it appears that the pus, on being transfuſed into the latter, undergoes a conſiderable modification, and becomes in ſome meaſure eſſentially changed. The facts adduced by Jenner appear to give ſufficient weight to his conjectures, and to juſtify the concluſion that the Cow-pox may draw its origin from the ulcers of horſes. The following obſervations may, nevertheleſs, throw ſome dubiety on theſe conjectures. Jenner's explanation of the fact is plauſible enough with regard to the province of Glouceſter, where the ſame perſons are entruſted with the care of both horſes and cows; but in Lombardy, on the large farms, where the herds of cattle are very numerous, the departments of the attendants are divided, the cows are committed to the charge of one claſs of perſons, and the horſes to that of another; and theſe two claſſes do nothing in common, nor is the one ever employed to diſcharge the offices of the other. Ireland, ſays Jenner, which abounds ſo much in cattle, is, till this hour, free from the Cow-pox; and the reaſon he gives for this exemption is, that the cows are milked only by women, and that a man would reckon himſelf diſhonoured were he employed in this office. The combination of theſe circumſtances among them is in oppoſition to what is obſerved in Lombardy, where, notwithſtanding that different perſons are employed about the cows and horſes, the former are ſubject to this diſtemper. There can, therefore, no ſtreſs be laid on the conſequences which Jenner would deduce, and his theory can only be regarded as an hypotheſis not properly ſubſtantiated. The origin of the Cow-pox then muſt neceſſarily be ſought from ſome other ſource; and if, after all, neither obſervation nor reaſoning be able to diſcover it, it muſt remain in the ſame obſcurity with many other ſubjects of diſquiſition which have fruitleſsly and without any utility occupied the attention of theoretical phyſicians and other learned men. Facts, alone, ought to be received as a confirmation

of

of the conjectures which propose to deduce the origin of the Cow-pox from equine ulcers; and these facts ought to be universal and constant. The only method to confirm or overturn such theories is, to inoculate cows with the pus of the grease, in the same manner as men are inoculated with the Cow-pox. A repetition of the experiments will shew whether it be contagious, and whether it really passes from the horses to the cows, or whether the latter come to be attacked with it in consequence of some other unknown combination. Jenner, as far as I know, has made only one experiment, and has not obtained the result, which his theory seems to require, of producing the Cow-pox. In order, then, completely to confirm the theory of Jenner, or directly to confute it, I undertook to make divers experiments on a certain number of cows; four were the number set apart for this attempt. I began with making some incisions with a lancet in the udder and nipples of one; I then introduced the fresh matter of the grease, which an expert groom, of Milan, had purposely procured me that very morning. As the animal was very restless during the time of the experiment, I deferred till next day the inoculation of the other three, wishing to avail myself of an instrument used in Germany by phlebotomists, as by means of it I could render the transfusion of the matter of the grease into the cows more frequent, more easy, and more decisive, on account of the multiplied apertures. Having charged the instrument with six lancets, I applied it at the roots of each pap, and by this means produced in all twenty-four small incisions, which became gently tinged with blood. Not only the place where the incisions had been made were filled with the grease matter, but the whole paps were covered with it. I inoculated the other two in the same manner with the greatest facility. I flattered myself that by this method the absorption of the pus could not fail to take place and to produce the desired effect, as it was rendered powerful both by the number of the incisions and the quantity of matter. Having covered the wounds where the pus had been introduced, with a piece of linen, around which pitch was laid, so that the matter might not be exposed to the air, or in any way removed from the place; contrary to my expectation, there was no sensible appearance on any of the cows, and the incisions dried up in two or three days, without producing any alteration whatever. Perhaps, the non-appearance of the effect I looked for may be attributed to the season of the year, as the autumn, in which I made the experiments, is not the most favourable to exanthematous eruptions, the contrary of which is observed of the spring. Perhaps there was wanting a predisposition to contract the distemper; or, perhaps, there was wanting some circumstances, unknown to us, but necessary to make it unfold itself. Perhaps, and it is a more probable supposition, the effect was not attained, because the grease is not the origin of the Cow-pox. I ingenuously bring forward my results, although I do not place implicit confidence in them, but prepare myself to repeat new experiments at different seasons of the year, which other medical men have also

alſo an opportunity of doing, in order to be convinced, whether they ought to receive or reject the conjectures of Jenner Let me add to the cauſes aſſigned to the Cow-pox, the opinion of thoſe who deduce it from a different origin, and regard it as originating in the cows themſelves, and proceeding from a particular ſtate in which they are at times found. It has been obſerved, that the diſtemper uſually appears in cows which have newly calved: from thence it is ſuppoſed that the milk, not finding the proper veſſels ſuddenly prepared for its diſcharge, becomes acrid, irritates the dugs, occaſions inflammation, and ſo produces this diſtemper. And, beyond doubt, when the milk is retained in the udder beyond the period aſſigned by nature, it contracts ſtimulating qualities, from which they may ſuffer; I do not however believe, that this circumſtance can give riſe to the Cow-pox, which is a contagious diſtemper. The theory is nowiſe ſupported by obſervation, as cows in the ſtate here deſcribed are to be found in every place. Another argument againſt this hypotheſis may be drawn from the analogy of thoſe ladies who follow the barbarous faſhion of not ſuckling their own children, and who of courſe retain the milk in their breaſts even till it acquires a great degree of acrimony. Although in ſome of them there have been obſerved exanthematous eruptions producing thick incruſtations and ſevere pains, there never aroſe from theſe ſymptoms, in one inſtance, a diſtemper of an infectious kind. I will not leave this ſubject without giving my own ideas, founded on many obſervations, and already advanced by many other medical men on the origin of exanthematous diſeaſes, among which both the Small-pox and Cow-pox muſt be comprehended. This theory ſuppoſes that ſuch diſeaſes are produced, or at leaſt always accompanied, by worms, which inſinuate themſelves into the ſkin, and afterwards neſtle in the puſtules, which grow up and come to a ſort of maturity. Etmuller and Hoffman, among the older writers, are of that opinion with regard to many diſeaſes; and ſome of the moderns, founding their ideas on incontrovertible microſcopic obſervations, look upon the itch and other contagious diſeaſes as originating from worms. Why may not the variola be of the ſame nature? I have hitherto had no opportunity of making thoſe reſearches on this ſubject which I have of a long time reſolved to engage in; as I have not been able to procure microſcopes of ſuch power as to enſure certain reſults. It is well known that an excellent method of cure in diſtempers from worms is the uſe of mercurial medicines, which have alſo been employed with ſucceſs in the itch. When applied to the variola it ought to have the ſame effect, provided this fatal and contagious diſtemper can be attributed to worms which are lodged in its puſtules. The celebrated Roman phyſician, Dr. Lapi, has publiſhed in an excellent work, his ſucceſsful experiments on the uſe of mercurial ointments in the variola, without however mentioning to what cauſe he attributed this effect, and without generalizing his theory and extending it to other exanthematous diſeaſes which are not contagious; to theſe it would have been proper in him to have applied the ſame treatment. I have myſelf repeated ſimilar experiments on the variola in a few

cases, and the result was conformable to that obtained by Doctor Lapi."

Before proceeding to state the cases of Vaccine Inoculation which occurred in his own practice, Dr. Sacco gives the following account of the manner in which he procured the pus for inoculation. "I had for some time been extremely desirous to repeat the experiments of Jenner, and for this purpose made diligent researches to discover the Cow-pox in Lombardy, it being extremely difficult, especially in the present circumstances, to obtain the pus from England. A fortunate combination of circumstances, by which it became necessary for me to go to the large town of Varese, in the beginning of autumn, procured me an opportunity of examining a number of cows on their way from Switzerland to the fair of Lugano; and by this means I had a favourable opportunity to make such researches, as might discover in some one of them the Cow-pox. It was on this ocasion, that conversing with some dealers in cattle, and countrymen who had large dairies in lower Lombardy, I learnt that the cows among us are subject to the Cow-pox. In this enquiry I took care to propose my questions in such a manner, as to prevent the risk of being imposed upon. A farmer of Cremona, who had bought forty cows in Switzerland, and had driven them from thence as far as Varese, assured me that almost all of them had been successively attacked with pustules on the extremity of their nipples, and some of these were now converted into incrustations. I visited the cows, and had an opportunity of verifying his assertions. I picked off some of these encrustations, with an intention of applying them in fomentation, if, perchance, I could not procure the true pus for inoculation. The same farmer promised me an opportunity of seeing this disease with my own eyes, and for this purpose conducted me to a neighbouring meadow, in which we found a herd of cows belonging to a friend of his. We examined these cows, and discovered on two of them different red spots, which the farmer assured me was the first stage of the disease; no other symptom appeared on the cows, but a slight degree of dejection. He assured me that this was the very disease I was in quest of, and that in the course of two days, the pustules would unfold themselves. At this visit which I made to the cows, there was present a dealer in the Grisons cows, who fully confirmed the truth of these assertions. He also added, that in his country he had seen the cows afflicted with a similar eruption on their dugs, and to remove the incrustations it was common to anoint them with boiled oil used for varnish; and that by this means they fell off in the course of two or three days. Early next morning I went again to see the cows, examined them anew, and found on one of them four red spots already tumid and raised into pustules; three of these were spread over the nipples, and the fourth lay in the middle of the dugs. The other cow had six pustules, two on the nipples, and the rest scattered above them. These were larger than those of the first cow, and around them appeared a slight

slight red circle. Apparently these pustules occasioned much pain to the cows, for on my approaching to examine them more minutely, they would scarcely permit me to touch them for one moment. Although the pustules were already large and prominent, they did not yet appear to me sufficiently mature to yield the matter I wanted. As the cows were that day to go forward on their way to Milan, I found myself under the necessity of following them to their first halting place, in order to examine them again next day. I walked out at an early hour to the meadow where they were at pasture. I examined the pustules, which appeared to me to be now arrived at maturity. They were lucid, and of a pale red colour, with a brown spot in the middle more depressed, and I thought this a favourable moment to collect the matter, which, through the assistance of the herdsmen, I was easily enabled to do by repeatedly soaking a thread in it. Although I saw no reason to doubt that this was the true Cow-pox, yet this being the first time I had ever seen it, I began to suspect that the pustules might be of that kind which Jenner calls the *spurious* Cow-pox; I determined therefore to decide the matter by experiment. A considerable number of experiments, all uniform in their symptoms and progress, and always constant in their results, put the matter beyond doubt, and gave me full conviction that this was the true Cow-pox. Such and so many are the obstacles to be overcome on the introduction of any innovation, however salutary, that I for some time despaired of being able to induce any one to submit to inoculation with the matter I had collected. In fine, after many fruitless persuasions, I succeeded in my design; the success which attended the first inoculations encouraged others to submit to the same process."

Dr. Sacco then proceeds to detail three hundred cases, in which he applied the pus he had obtained in the manner described above. These cases were attended with various circumstances; but the inoculation succeeded to produce the Cow-pox in all of them; and in a considerable number the inoculation for the Small-pox was afterwards applied, but without any effect. Having learned from a veterinary surgeon in the department of the Eure and Loire, that not only oxen, sheep, and rabbits, but dogs also were subject to the effects of the Vaccine inoculation, Dr. Sacco extended his researches likewise to this subject, reckoning no part of physical knowledge a fruitless or barren disquisition. He states the results he obtained as follows. "I took seven dogs, of different species, and subjected them successively to the Vaccine inoculation, constantly employing the matter obtained from the preceding to inoculate the one I next intended to submit to the operation. The disease was produced on them, but had a much shorter period than it usually has in the human species. Two of the dogs were for the space of one day extremely dejected, and refused all manner of food; but during the rest of the period they appeared perfectly well, nor was there any other consequence that merits observation, except that the Vaccine Inoculation transmitted

from one dog to another preserved always the same force, and never lost its activity.

"In the first five of these experiments I applied that for the Small-pox after the Vaccine Inoculation; I had not an opportunity of trying the same operation on the sixth, as he had to attend his master on a journey. On the seventh, some time after the incrustations had begun to fall off, suspicious symptoms of the hydrophobia began to make their appearance; his habitudes underwent a rapid change, he became unwilling to drink, bit some persons, and in consequence was put to death. None of the persons he had bitten were attacked with hydrophobia. Such were the consequences of the inoculation of the Small-pox practised on dogs after the Vaccine. I afterwards inoculated the Small-pox on two dogs who had not been previously vaccinated; and one of them had three pustules on his lips, and one in the vicinity of the inoculation; on the other, one only appeared in the neighbourhood of the inoculation. During the course of the disease, these dogs were for some days melancholic, and ate much less than usual. From these experiments it appears, that the Vaccine Inoculation acts as a preservative in dogs against the Small-pox. The symptoms which I observed in my Vaccine inoculation of dogs do not enable me to form a precise judgement, whether it is attended with any inflammation, great or small, on the lungs; so that I am unable either to oppose or coincide with the sentiments of the illustrious English physician on this subject."

The Doctor then proceeds to give an account of the method of inoculation, and the treatment of the Cow-pox, but adds no new observations to the compleat information already possessed in England on these subjects. An extract from the consequences and reflections he has deduced from his experiments and observations will probably be acceptable to the English reader.

"1st. The Cow-pox appears to be a distemper incident to every species of that animal, since it is not confined to England, but is also known in Holstein, is extended to Switzerland and Lombardy, and we may conclude, without hesitation, that it is also to be found in other countries. The cause that has brought this circumstance into doubt, is the ignorance of its merits, and the consequent inattention of intelligent persons. In fact, no one had the least knowledge of it in Lombardy, nor would even have suspected its existence there, if the researches which I undertook, following the footsteps of Jenner, had not succeeded in discovering it. I have now used that matter, which I found out in the manner above described, in such inoculations as I have been engaged in during the last eight months. Other medical men of Lombardy also, though they have not discovered precisely the same species of variola, nevertheless entertain the best founded suspicions that it exists among our cows; and from the replies which I have received to my enquiries on that subject, I am inclined to believe, that if attentive observers would employ themselves in this research, the common and general existence of this disease among our cows would soon be

be established. Moreover, the Cow-pox might be introduced and propagated without the necessity of bringing the pus from distant countries, as is the practice throughout all Europe, to every country of which it is brought from England with a success not always to be relied upon, since it loses its activity by the length of time that must necessarily intervene before it can be applied to use, and is thus in danger, in place of the true Cow-pox, of communicating the spurious, which does not preserve from the attacks of the Small-pox.

"2d. It appears that the vaccine variola ought to be regarded as a modification of the human, and if they differ in some degree with regard to the seat of the malady and the method of its communication, they agree in the remainder of their essential qualities. The human variola is communicated from one subject to another by the means of miasmata invisibly transported through the air; it is communicated by the mere contact of sound persons with the infected, and even by handling the clothes of those who have the disease. The vaccine variola, on the contrary, is only communicated by means of insertion; by directly bringing the matter of it into contact with the mass of the blood by means of slight incisions or punctures, or by strong and long continued friction. This last is the means by which it is communicated to those who are employed in milking the cows; the operation warms and opens the pores of their hands, by which means the pus that is squeezed by compression out of the pustules has an opportunity of introducing itself through the absorbent vessels at the extremity of the capillary veins. The human variola discovers itself on every external part of the body without exception, and even on the internal, as the mouth and jaws; while the vaccine may be considered as purely local, that is to say, limited to the margin of the incisions, or punctures, through which it was introduced; and there alone it produces a hardness, an inflammation, and pustules. The distance of time between the inoculation and the appearance of the disease is not constant, either in the human or vaccine variola. In both, the first symptoms begin to appear about the fifth day, or the days immediately succeeding; sometimes not till the thirteenth, the twentieth, and even the fortieth day.

"3d. It appears a rash assertion of Jenner, that the grease of horses is the exciting cause of that disease in cows which is called the Cow-pox. I have proved the falsity of this opinion by direct experiments and observations; and I have since been informed that Pearson, Simmons, and Woodville likewise entertain sentiments on this subject opposite to those of Jenner.

"Jenner wished to extend the effects of this matter so far as even to consider it as the general cause of all contagious diseases; and in order to render the conjecture in some degree plausible, he believes that this matter, upon being modified and changed in the human body by certain compound and unknown means, may afterwards unfold its activity in some Protean form. But as it has been demonstrated that the simple matter of the grease, which is regarded

regarded by Jenner as the exciting cause of the Cow-pox without any modification, is not so in any way whatsoever; it ought with still more reason to be excluded from being the origin of other contagious disorders; a theory without the smallest proof or foundation.

4th. From numberless experiments, it may be looked upon as a demonstrated maxim, that whoever has had the Cow-pox is no longer subject to the attacks of the Small-pox.

"5th. I look upon it as also demonstrated, that the true Cow-pox cannot be produced in those who have had the natural Small-pox. I have above detailed different experiments which I have made with a view to this question: I will now add that the celebrated Dr. Arragoni, who had in his early years the confluent Small-pox, made me repeat on him the Vaccine inoculation with fresh matter, and with all the precautions necessary to produce the true Cow-pox; but the only results obtained were those of the spurious. It is a general received opinion in England, as different English Inoculators have advanced in their works, that those who once had the natural Small-pox are no longer liable to the Cow-pox. I know that Jenner and Pearson are of a contrary opinion. Few are the facts on which they rest their theory, and those few are only such as Jenner has produced in his first work. My own experience, nevertheless, and the many observations I have made, confirm me in my own opinion, and furnish me with a criterion by which to determine whether those, who doubt if they have had the natural Small-pox, have or have not been attacked with it.

"6th. Where the proper effect has been produced on those vaccinated, they are no longer capable of contracting the disease a second time. This conclusion is in opposition to the opinion of Jenner and Pearson; but to their authority I oppose the results of my own experiments, and those of other inoculators. Besides the different attempts which I made to vaccinate different subjects a second time, I also subjected myself to the same experiment: but the only result I obtained, alike from them and myself, was the spurious Cow-pox, of a short period, and producing in some of them a slight fever. In this particular the human variola agrees with the vaccine, as both of them, on a second inoculation, produce only transitory symptoms.

"7th. From above two thousand vaccinated persons having lived with those who were not vaccinated, yet without communicating the disease to the latter, we may conclude that it is not contagious in the human species. Woodville alone holds a contrary opinion; but as he adduces only two facts in opposition to so many, it is probable he has been misled by an anomalous appearance of the natural Small-pox during the course of the Vaccine inoculation.

"The true Cow-pox is by no means to be confounded with that which has been denominated the *spurious*. It would be a fatal error to employ the one as equal in effect to the other. The *true* preserves from the Small-pox, the *spurious* does not. These two diseases have their distinctive characters; and I will not scruple to repeat,

repeat, that it is of the utmost importance to be able to distinguish them at first sight; this alone can form that tactus eruditus which is the result of repeated experiments, of some errors, and of accurate observations. It will not therefore be superfluous to repeat the observations I have made on this article. The true Cow-pox does not shew itself before the 4th day; sometimes not till the 7th, the 8th, or even later. Its first symptom is the appearance at the place where the punctures have been made, of a spot like that which is occasioned by a flea-bite, with a very minute elevation towards its centre, as may be seen in Figure 2, Plate II. Two or three days after the appearance of the afore-mentioned redness, this elevation augments in diameter, and takes the form of a small pustule or tumour. The red spot increases in proportion, and now forms a ring of larger circumference, as may be seen in Figure 3. About the 5th or 6th day it arrives at its completion. It is of a circular figure, and its diameter is from three to five lines; it has elevated borders of a clear ash colour; its centre, which is of a reddish brown, is depressed and filled with a limpid crystalline matter. It now becomes surrounded with a red ring of a larger diameter than that of the preceding days, (as may be seen in Figure 4:) this ring continues to expand itself with increasing rapidity, acquiring a magnitude very well represented by Fig. 5, and embanked as it were by a slight miliary eruption. At the same time the red colour undergoes various degradations, being always most intense in the most eccentric parts. Such parts as are reddish at this period, especially in the vicinity of the pustule, contract a hardness in the cellular membrane similar to what takes place in an erisypelas. An incrustation commonly succeeds in place of the pustule, without any suppuration; and at the same time the erisypelous tumour begins by degrees to wear off; the incrustation preserves in every part the exact figure of the pustule, and presents a surface, bright, smooth, and as it were pellucid, of which some idea may be formed from Fig. 6. Towards the third or fourth day of the appearance of these symptoms, the persons vaccinated are attacked by some degree of fever, accompanied by debility and want of appetite. The duration of the febrile state is usually confined to a few hours; in some persons it returns a second time, but very seldom is again renewed. At the same time that the fever makes its appearance, there begin to be felt pains under the arm-pits, attended with a tension of the glands, which symptoms regularly disappear when the fever goes off. It sometimes happens, that after the formation of the pustule is completed, it opens and suppurates. In that case the incrustation by which it is covered has an unequal surface, an unequal colour, variegated with yellow and red, and is likewise of a large size, as may be learnt from Fig. 7. The duration of both these incrustations is about eight, ten, and sometimes fifteen days before they become disengaged from the skin. They sometimes fall off, and are re-produced again and again, always occasioning some suppuration. This description comprehends a case of the Cow pox accompanied with well-formed pustules, and which therefore cannot

be

be miſtaken. It frequently happens, that ſuddenly, and in the courſe of a few hours after the inoculation has taken place, eſpecially if it has been performed by means of threads, or with matter which has been long kept, the firſt ſymptoms begin to appear, inflammation at the ſeat of the puncture, the rapid formation of a puſtule containing matter, and moſt commonly alſo a ſenſible fever accompanied with vomiting. The puſtule opens of itſelf, and in the ſpace of about ſix days after the operation has been performed, the exſiccation ſucceeds; no veſtige of the puſtule then remains except a ſlight incruſtation, which often continues to ooze out matter for a long time afterwards. Such are the ſymptoms that excite ſuſpicion of their being derived from the ſpurious Cow-pox. I have applied myſelf with that ſolicitude which the importance and difficulty of the ſubject required to form a preciſe diagnoſis, which may render us ſecure of not miſtaking the ſpurious for the true.

"If after the above-mentioned ſhort period, the diſeaſe entirely vaniſhes, it is certain that the ſymptoms which have appeared are thoſe of the ſpurious. On the contrary, if, after that ſhort period, the inflammation takes place anew around the encruſtations; if, in place of them, an elevation and ſwelling are formed in the cellular membrane, with an external eriſypelous appearance; if on the falling off of the encruſtations the wound opens afreſh and begins to ſuppurate, and has nearly every ſymptom that uſually accompanies the true Cow-pox; we may conclude that the true has been obtained, its ſymptoms having ſucceeded to the former dubious ones. The ſpurious Cow-pox is ſuch as has been deſcribed above. The moſt eſſential of its indications is the great celerity with which it appears after the innoculation has taken place, while the ſymptoms of the true do not make their appearance till after four, five, or more days. The encruſtations of the ſpurious are ſmall and irregular; while thoſe of the true are much larger, of a regular figure, and relucent. It will be aſked, What is the origin of the Spurious Cow-pox? From what Jenner ſays, and from what I have myſelf obſerved, it takes place whenever the matter for inoculation is extracted from puſtules originally ſpurious, ſuch as I have already noticed in my deſcription of the Cow-pox: It takes place when the matter is from puſtules already approaching to exſiccation, and in a ſtate of ſuppuration: It takes place when the virus uſed for inoculation has been long kept, and altered by heat or any other cauſe, and therefore little reliance is to be placed in that tranſmitted from diſtant countries, unleſs it arrive quickly at the place of its deſtination: It takes place when the matter of inoculation is taken from that already known to be ſpurious from its courſe: It takes place whenever the true kind is uſed in the inoculation of any one who has already had the natural ſmall-pox, or who has already been vaccinated. Thoſe who wiſh to know the reaſon of every thing, will expect to learn from me, why, in theſe laſt caſes, inoculation with the matter of the true Cow-pox produces only ſpurious, by which means a criterion is formed

The appearance of the Vaccine pustules on the udder of a Cow.

Vide p.185 of Vo

A.B. *The appearance of two pustules produced by an inoculation of the Cow.*

a.a.a.. *The Pustules that are formed naturally.*

formed to judge whether Vaccination or the natural Small-pox has already taken place. I confess my ignorance in common with all those who have already published the same observations, and we must still wait an explanation of the phenomenon from time, from some fortunate accident, or from new observations that may rend asunder the veil of this medical mystery.

Dr. Sacco subjoins to this work a plan to render the use and the advantages of the Cow-pox general. The methods which he points out for this purpose are, 1st. To form a public institution for inoculating gratis, situated in the capital, and communicating with similar subordinate institutions to be established through all parts of the kingdom. The principal institution to have the charge of supplying any deficiences of the matter of inoculation, and also of circulating any new improvements or farther discoveries of the diagnosis and treatment. 2d. All admitted into the Foundling, Orphan, &c. and all other public hospitals and institutions, to be vaccinated immediately on their admittance, or on one day of every week set apart for that purpose. 3d. No medical man shall be admitted to any employment in the public service, without first entering into an obligation, on pain of forfeiture of his office, to vaccinate all those *gratis* who shall apply to him for that benefit. Such are the leading principles of Dr. Sacco's plan: the detail of it is adapted entirely to his own city of Milan and the surrounding country, and therefore could yield no information of general utility. He concludes with a hope, that the adoption of this plan would speedily introduce a custom of vaccinating all new born children, and thus in a short time deliver the world from that dreadful malady the natural Small-pox.

☞ *The interesting Plate representing the Cow-Pox as it appears in Lombardy on the Udder of a Cow, was not ready in time, owing to the neglect of the Engraver, but it will certainly appear in the next Number.*

Observations et Experiences sur l'Inoculation de la Vaccine, par Jean De Carro, Docteur en Medecine. 8vo. pp. 216. Vienna, 1801.

The Author of this excellent work on the new Inoculation may be considered as the principal founder and promoter of it in Germany, Russia, and Turkey. In the Dedication to Lord Minto, our Ambassador at Vienna, he says, "The healing art is indebted to England for a multitude of useful discoveries; no epoch, however, has been so memorable, nor has promised to mankind advantages so important as Vaccine Inoculation, the immediate tendency of which is to annihilate that destructive scourge the Small-pox.

"England already acknowledges Jenner to be one of her greatest benefactors, and all civilized nations hasten eagerly to profit by his admirable discovery. Having had the good fortune to be the instrument of its introduction into the Austrian monarchy, I owe to

to the repreſentative of Great Britain the homage of my firſt ſucceſs."

In his Preface he announces the end propoſed by the publication of this work to be, "To collect the proofs of the preſervative power of Vaccine Inoculation, to lay them before the public in a methodical and accurate form, to examine the opinions of the moſt celebrated inoculators on all the intereſting points relating to the Theory and Practice of Vaccination, and not to give my own opinion, ſays Dr. C. till after the reſult of numerous obſervations and experiments which I have had occaſion to make upon this new inoculation.

"In order to facilitate as far as poſſible the circulation of a work, written for parents as well as medical practitioners, I have had it tranſlated into German, under my own inſpection, by Dr. De Portenſchlag, junior."

Our readers will judge of the *extent* of this elaborate work, by the following heads of the ſeveral chapters.

1. Remarks on the name of the diſeaſe. 2. Hiſtory of the diſcovery of the Vaccine Inoculation. 3. Origin of the Vaccine. 4. Deſcription of the true appearance of the vaccine puſtule, with a plate. 5. On the poſſibility of having the diſeaſe repeatedly, and the Vaccine after the Small-pox. 6. Can it be communicated by any other means than inoculation? 7. Obſervations on the eruptions which have been obſerved in the Inoculation Hoſpital at London, and ſome other places. 8. Is it difficult to propagate the diſeaſe, and keep up a ſupply of the virus out of England? 9. An account of the Inſtitution for the Inoculation of the Vaccine in England, and rereflections on the neceſſity of ſuch eſtabliſhments. 10. Is it neceſſary that there ſhould be a decided fever to be aſſured of the anti-variolous power of the Vaccine? 11. Are the Vaccine and the Small-pox the ſame diſeaſe differently modified? 12. Of the immediate and remote advantages of the Vaccine over the Small-pox. 13. Are there any well-founded objections to the adoption of the Vaccine? 14. Are there any authentic caſes where the Small-pox has appeared after the Vaccine? 15. Obſervations on the practice of the new inoculation. 16. A liſt of my inoculated patients.

To which is added an appendix of ſixteen pages, containing letters from his correſpondents.

In his ſecond chapter on the hiſtory, he ſays, "I ought not to forget the correſpondence with my Lord Elgin, Ambaſſador Extraordinary of his Britannic Majeſty, at Conſtantinople. I have had, through his means, the pleaſure of contributing to pay a part of the debt we owe the Turks for teaching us the inoculation of the Small-pox. My Lord Elgin, convinced of the importance of Dr. Jenner's diſcovery, and the authenticity of the facts upon which it was founded, applied to me for virus, to inoculate his only ſon Lord Bruce, in which he ſucceeded compleatly." Then follows a copy of Lord Elgin's letter to Dr. De Carro, on which he remarks. "Here we ſee an Ambaſſador from England, acting the ſame part with the Vaccine in Turkey, as an Ambaſſadreſs (Lady W. Montague) had acted in Europe on her return from Conſtantinople.

"Laſtly,

"Lastly, Of all the proofs which I could give of the high importance which the English nation attaches to the discovery of Dr. Jenner, none appear to me more decisive than the order given by the Admiralty for the inoculation of all the children of sailors, and the sailors themselves who had not undergone the Small-pox; not only in the ports of Great Britain, but in the most distant settlements. When we reflect for an instant, that this class of men constitutes the power and the glory of that Nation, can we doubt the advantages, or the authenticity of this inoculation?"

Dr. De Carro's plate represents the pustule something smaller than it appears in London; and we are informed that it is smaller in England, in the country, than in London.

In his chapter on the eruptions, he supports the opinion that is now, we believe, generally received here, viz. that they have nothing to do with Vaccination. He concludes it thus, "I am at present so convinced of the safety of the Vaccine, that I pay no attention to the state of the child's health before inoculation. The weakest as well as the strongest go through it with the same ease. In a word, I repeat here what I have so often said to apprehensive parents, *If a child is not dying of some other disease, he will not die of Vaccine.*"

Near the end of his eighth chapter, on the means of continuing the true virus, "How many country practitioners, who were not sufficiently acquainted with the disease, rashly inoculated with what they considered as the true Vaccine, but which was spurious. So that I strongly recommend all persons who value this discovery, to distrust all unfavourable reports made by people who do not give the genealogy of their virus. This precaution is the more necessary, because I certainly know that some interested enemies of the new method, have several times tried with virus, which they inserted as vaccine, but which was spoiled by heat, or designedly mixed with variolous matter, in order to produce consequences which might create a general disgust against Vaccine Inoculation."

On the question respecting the necessity of a constitutional affection or fever as forming a part of the genuine disease, Dr. De Carro declares, "that whenever the vaccine pustule, or rather vesicle, assumes its true character, which he says may be ascertained by the plates of Dr. Jenner, or those in the present work, we may safely conclude that the patient will be for ever safe from the Small-pox."

We cannot help regretting on this occasion that Dr. Jenner's engagements prevent him from giving to the public those very accurate and beautifully coloured plates which he is now preparing to accompany the next edition of his works on Vaccination. Those plates would indeed be a rudder and a compass by which the practitioner might steer with safety.

"After the opinion I have just given, (says Dr. De Carro) how can it be conceived that an effect apparently merely local, can guard against such a disease as the Small-pox, whose effects on the general system are so violent and so well known? Certainly, the fact

fact is very extraordinary; it is a new mystery added to those which, from the commencement of medical science, have been deplored by its Professors.

"Few people, it appears to me, can suppose that the variolous action is merely local, since such numerous experiments in various climates prove the contrary. I believe that a constitutional affection does always exist, but that it is so slight as often to escape the notice of the inoculated and the Inoculator. According to my observation, in those families where the children have been watched with the greatest attention and care, the most constant symptom has been restlessness during the night of the 9th and 10th day. It is obvious, that symptoms so slight as these will escape the notice of the generality of parents, and more so that of physicians, who are so seldom with their patients at those hours. But though no symptom of fever has been noticed, we cannot conclude that it had no existence. Fortunately for the success of this inoculation, we certainly know, that the appearance of the areola, is a decided proof that the anti-variolous change has been produced in the system; and independent of this circumstance I know no means by which the practitioner can be assured that the true disease has taken place.

In the eleventh chapter he draws this conclusion. "Dr. Turton is the only one who has concluded that Variola and Vacciola are the same disease differently modified; which opinion, I think, is sufficiently refuted by what I have advanced above. It is more reasonable to suppose that they are distinct diseases, the one originating in the cow, the other in man; but that they have a certain relation which cannot be described nor explained. The discovery is a new and unique fact in Physiology, Pathology, Natural History, and Veterinary Science."

Dr. De Carro concludes his next Chapter in these terms. "In the course of the last autumn and winter, five cases of Inoculation occurred in children who had before received the infection of the natural Small-pox, and in whom that disease appeared a few days after the Vaccine Inoculation. These five children had the Small-pox astonishingly mild, at a time when that epidemic was uncommonly severe. The two maladies proceeded together with the greatest regularity, and each preserved its peculiar character without approaching the other."

To the Editors of the Medical and Physical Journal.

GENTLEMEN,

FROM not reading the Medical and Physical Journal, for November, attentively, I did not perceive that you alluded to any remark I had made respecting Bilious Diarrhœa. On referring however to what was stated in the subjoined note to the List for that

that month, I acknowledge not to have expreſſed myſelf with ſufficient clearneſs. It no doubt has been obſerved in the Liſt that there were ſome caſes which I viewed as Idiopathic Dyſentery, becauſe not obviouſly connected with any morbid ſtate of the liver; in theſe caſes then it frequently happens that ſcybala are diſcharged. But in the Synochus Bilioſa Dyſenterica, where for the moſt part bile paſſes off in an increaſed quantity and vitiated ſtate, I believe ſcybala are very rarely perceived. In general alſo, in the latter affection, the quantity of blood is much greater than in the former, although attended with a leſs degree of teneſmus.

The Typhus Gravior, which I have mentioned in my account of the bilious fever juſt publiſhed, was at its acme the latter end of December, 1800, and diſappeared in January, 1801. I am, &c.

Bath, Jan. 11, 1802. W. WHITE.

List of Diseaſes at the Bath City Diſpenſary, from December 1, 1801, *to January* 1, 1802.

Angina	1	Peripneumonia Notha	7
Aſthma	7	Pulmonary Complaints	4
Anaſarca	3	Phthiſis	6
Bilious Complaints	2	Phlegmone	1
Bloody Urine	1	Pertuſſis	2
Contuſion	3	Podagra	1
Catarrh	17	Phyſconia	2
Cholic	1	Pſora	1
Chloroſis	1	Quotidian	1
Eryſipelas	2	Rheumatiſmus Acutus	9
Excreſcence	1	Synochus Bilioſa	7
Febris Simplex	2	——— Gaſtritica	1
Fiſtula in Perinæo	1	——— Cholerica	1
Gravel	1	Suppuration	2
Gaſtrodynia	3	Syphilis	5
Hernia Humoralis	1	Scrophula	2
Hernia	2	Strain	1
Leucorrhœa	1	Tertian	1
Lepra	2	Tuſſis	3
Nephritic Complaint	1	Ulcerated Legs	7
Ophthalmia Tarſi	1	Variola Diſcreta	1

N. B. In November Statement, inſtead of *Cyst* read *Cystorrhœa*.

MEDICAL AND PHYSICAL INTELLIGENCE.

[FOREIGN AND DOMESTIC.]

Dr. Schmidtmann, of Osnabruck, has given in Hufeland's Journal, an account of the imposition in the Case of nearly two years abstinence from all food and drink, which we related in one of the former numbers of this Journal upon the authority of several publications that had appeared on this subject, particularly of a pamphlet written by the above gentleman, in which the whole case was stated, and some remarks added respecting the possibility of such a long abstinence, which the author endeavoured to countenance by appealing to the records of similar cases, and by theoretical reasoning, according to which he fancied the patient might have drawn sufficient nourishment from the elements of the atmosphere, particularly from the oxygen. The subsequent discovery, however, will show how much the Doctor's learning and sagacity had been misplaced. Reports were even then spread of the whole being an imposture, in order to excite pity, and to extort presents from the people who went to see this miraculous girl. Though watched by attentive men, she had found means to deceive them, till at last some young lawyers and physicians took the pains of guarding her for several days, and by their great attention, they succeeded in discovering the patient to have excretions of urine as well as of fæces. Though she abstained during four days from food and drink, her violent thirst at last overcame her resolution; and notwithstanding she refused taking any solid food for eight days, at last she was obliged to ask for it. Upon a strict examination before the magistrate it was found, that she had, indeed, for a considerable time, abstained from taking any nourishment; but instigated by some of her acquaintances to take the advantage of the credulity of the public for the purpose of gaining money and other presents, she had first conceived the idea of feigning a total abstinence from food and drink, though she was all the time secretly provided with both by her brother, a boy about twelve years, and with the knowledge of her mother. It remains, however, remarkable in this case, how the patient had subsisted upon such scanty provisions as her brother could provide her with, without a proportionable decrease of flesh and strength; and it is curious to observe, that the auri sacra fames could make of a simple country girl, a most artful impostor. For this imposture the girl was committed to bridewell, and the mother and brother, as accomplices, received adequate punishment; but the father was acquitted, as no proofs could be produced of his having shared in the imposition.

Mr.

Mr. ZURINE, of Geneva, Aſſociate of the National Inſtitute, has preſented ſome curious remarks on a ſmall inſect, known under the name of water-flea (*monoculus pulex*, L.) which ſometimes reſides in the greateſt abundance in ſtanding water, and has frequently given riſe to the rumours of bloody rain, becauſe the great quantity of eggs which it lays in the ſpring, imparts a bloody colour to the water. Although the beſt naturaliſts had made this ſmall animal the object of their reſearches, ſuch as *Swammerdam, de Geer, Schæffer*, &c. yet Mr. *Zurine* diſcovered in this ſmall inſect very remarkable properties in its ſtructure, oeconomy, manner of coupling, &c., which had eſcaped the notice of thoſe literati. The moſt remarkable obſervation in theſe animals is, that the females, when once ſufficiently impregnated, communicate their faculty of propagation to ſix ſucceſſive generations, a phenomenon which has alſo been obſerved by *Bonnet*, in the family of Plant-louſes, (*aphis*, L.)

Dr. BATTY will commence his Courſe of Lectures on the Theory and Practice of Midwifery, and on the Diſeaſes of Women and Children, on Monday the 8th of February, at eleven o'clock in the forenoon. Further particulars may be known at his houſe in Great Marlborough Street.

Leadenhall Street, Jan. 28, 1802.

A report having been circulated, that the Lectures of Dr. POLE and Mr. WAGSTAFFE on the Theory and Practice of Midwifery and the Diſeaſes of Women and Children, would be diſcontinued; they take this opportunity of declaring ſuch report to be unfounded; that they commenced another Courſe on the 20th inſtant, and will continue to lecture at the uſual ſeaſons, of which public notice will be given.

Mr. MACARTNEY will begin his Courſe of Lectures on Comparative Anatomy and Phyſiology, early in this month, at the Medical Theatre, St. Bartholomew's Hoſpital. The Lectures will be given three times a week, and will terminate the end of April.

Mr. THOMAS will commence his Spring Courſe of Lectures on Surgery, on Monday the 1ſt of February, at ſeven o'clock in the evening. Particulars may be known at his houſe in Leiceſter Square, or at the Anatomical Theatre, Great Windmill Street.

CORRIGENDA et ADDENDA.

Vol. vi. p. 482, l. 14, from bottom, for Camph. ℥ij. read Camph. ℥ſs.
Vol. vii. p. 6, l. 10, read Phyſics.

A part of Mr. Dunning's Communications, laſt month, being by miſtake, ſent to different Editors, an omiſſion and conſequent obſcurity has crept into p. 5, l. 17. The parentheſis was intended to introduce the following Note, viz.

The Small-pox prevail now in this town and are very fatal:—Inoculation for them, which in itſelf is ſometimes very troubleſome, and now and then deſtroys, tends directly to ſpread and perpetuate this afflicting diſeaſe, while every caſe of INOCULATED COW-POX is, perhaps, one ſtep towards the EXTINCTION of it.

From an earneſt wiſh to forward this moſt deſirable and, now in all probability, attainable event, wholly uninfluenced by any pecuniary conſiderations, and wholly diſclaiming every view to popularity, at the hazard however I am afraid and am ſorry for it, of appearing in the opinion of ſome a little over zealous, and it may be irregular;

lar; I venture by dropping this paper among my friends and neighbours to call a little the public attention more immediately to the subject of the new inoculation, and, in this way to say, that I shall, at all times, experience the utmost satisfaction in inoculating, *gratuitously*, with Cow-pox, as many as I conveniently can, of those inhabitants in this town and parish whose circumstances will not enable them on this occasion to engage medical assistance:—And that they be encouraged to avail themselves of the great advantages to be derived from the new practice, I judge it proper to inform them that this easy means of prevention has been so fully ascertained to be an entire and lasting security against every possible exposure to the Small pox, the fact has lately been publicly subscribed by the greater part of the most eminent and respectable Physicians and Surgeons in London, and by many who had at first started objections to it.

That the celebrated Dr. PEARSON has observed, "That the chance of life appears to be greater during the Cow-pox Inoculation than under the ordinary circumstances of life; that he has in general found the health of sickly children mended by it, and does not know of any disease produced or excited by it."

That from this testimony, from many others nearly as strong, from my own experience, and I may add, from that of this neighbourhood, I can almost undertake to assert, that the degree of loathsomeness and distress usually attendant on a single bad case of natural, or even inoculated Small-pox, is greater than the whole amount of that which has hitherto arisen from all the cases, now more than ten thousand, of inoculated Cow-pox.

That as from the extreme mildness of this new practice no harm can possibly happen from it, it is become a custom with many, who cannot yet fully credit the efficiency of its protection against the Small-pox, to adopt it as a matter of course for their infants.

That this simple process, being little more than a local affection of the arm, seldom subjects to the least confinement or interruption in the management of a family.

That this disorder, if it can be called disorder, is not at all infectious, and that therefore infants at the breast, and their nurses, may be inoculated immediately in succession, or even at the same time, with little or no inconvenience to each other.

N. B. It is a confirmed observation that infants in the first months are but little liable to take on the action of the natural Small-pox, how much soever exposed to them; were they therefore, at this early period, regularly inoculated with Cow-pox, and inoculation for the Small-pox generally discontinued, (the great and obvious propriety of attending to both these circumstances will soon, I hope, be allowed) it would, by no means, be unreasonable to conclude, that the latter, having no longer but few subjects for its ravages, must necessarily in a few years disappear, and both inoculations, as far as relates to the Small-pox, consequently be superseded.

St Aubyn Street, Dock, Sept. 1800. RICHARD DUNNING.

Our Readers are therefore requested, in reading that page, to omit the parenthesis, and immediately after it, for than *read* and.

To CORRESPONDENTS.

We are sorry that Mr. Ward's request to have his paper returned for alteration did not come into the Editors hands till after it was printed off. As Mr. Ward appears desirous of avoiding unnecessary controversy, we entreat our readers to suppose the paper not published, till Mr. W. has made such additions and alterations as he may judge proper.

Mr. Mann's letter shall be sent as desired.

Mr. Cam's Case of Monstrosity has not been received.

The Indices are not made by the Editors, but they will be happy to correct any error pointed out to them. They could never doubt Mr. Hope's veracity or accuracy; and if the Index, or running Titles (which we again request our Correspondents to add to their own papers) convey Ideas different from those intended *by the Writers, it is not by design.*

R. M's difficulties in the Brunonian System are too metaphysical for our Journal.

G. W's request is inadmissible. To attempt to comply with it would be Quackery. An experienced practitioner, after due inquiries in person, can alone answer him.

Communications are received from Drs. Cogan, Brown, and White; Mess. Howard, Cooke, Crowthers, and Philosophus, for whose name Dr. B. will be obliged.